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CONTENTS.—See page V.

Some Astronomical Paradoxes.

By J. E. GORE, M.R.I.A., F.R.A.S.

THE term "paradox" is sometimes applied to the idea of a flat earth, the non-rotation of the moon on its axis, and other false hypotheses "contrary to received opinion." But the word is also defined as "a statement or proposition which seems to be absurd, or at variance with common sense, or to contradict some previously ascertained truth, though when properly investigated it may be found to be perfectly well founded" (Imperial Dictionary). It is in the latter sense that I propose to deal with some astronomical facts which, although at first sight apparently incorrect or even incredible, are, nevertheless, quite true.

It is probably a matter of common observation—at least, among those who have paid any attention at all to the heavens—that the stars are not all of equal brightness. St. Paul says: "One star differeth from another star in glory."* There are stars of all degrees of brilliancy, from Sirius down to a star just visible to the naked eye on a clear, moonless night. And below this brightness there are thousands and millions of fainter stars, some visible in an opera glass, others in small telescopes; while the faintest are only visible in the largest telescopes yet constructed. Now, it was a natural supposition, and one held for many years, that the brightest stars are invariably the nearest to the earth and that the fainter stars lie at greater distances. But measures of parallax show that this conclusion is by no means correct. It is true that the nearest star to the earth, α Centauri, is one of the brightest stars in the sky--third in order of brightness—but there are several comparatively faint stars which are nearer to us than some of the brightest stars. Thus the small star known as Lalande 21,185 of magnitude $7\frac{1}{2}$, and, therefore, invisible to the naked eye, is a little nearer to us than Sirius, and the bright star Capella is about five times farther from the earth than 61 Cygni, a star of only the 5th magnitude. The explanation of this apparent paradox is, of course, that the stars are not all of the same size and intrinsic brightness of surface; some are large bodies at a great distance from the earth, while others are comparatively small bodies, but much nearer to us.

The bright star Capella forms a curious anomaly or paradox. Spectroscopic observations show that it is a very close binary pair. It has been seen "elongated" at the Greenwich Observatory with the great 28-inch refractor—the work of Sir Howard Grubb—and the spectroscopic and visual measurements indicate that its mass is about 18 times the sun's mass. But its parallax (about $0''.08$) shows that it is about 128 times brighter than the sun! This great brilliancy is inconsistent with the computed mass of the star, which would indicate a much smaller brightness. The sun placed at the distance of Capella would shine as a star of about $5\frac{1}{2}$ magnitude. As the spectrum of Capella closely resembles the solar spectrum, the discrepancy between the computed and actual brightness cannot be explained satisfactorily, and the star remains an astronomical enigma.

Another curious paradox connected with binary stars has recently come to light. For many years it was almost taken for granted that the brighter star of the pair has a larger mass than the fainter component. This was a natural conclusion, as both stars are practically at the same distance from the earth. But it has been found recently that in some binary stars the fainter component has actually the larger mass! Thus, in the binary star ϵ Hydræ the "magnitude" of the components are three and six, indicating that the brighter star is about 16 times brighter than the fainter component. Yet calculations by Lewis show that the fainter star has six times the mass of the brighter! In 70 Ophiuchi Prey finds that the fainter star has about four times the mass of the brighter star. In 85 Pegasi the brighter star is about 40 times brighter than its companion, while Furner finds that the mass of the fainter star is about four times that of the brighter! And there are other similar cases. In fact, in these remarkable binary combinations of suns the fainter star is really the "primary," and is, so far as mass is concerned, "the predominant partner." This is a curious anomaly, and cannot be well explained in the present state of our knowledge of sidereal systems. In the case of α Centauri the masses of the components are about equal, while the primary is about three times brighter than the companion. But here the discrepancy is satisfactorily explained by the difference in the character of their spectra, the brighter component having a spectrum of the solar type, while the fainter seems further advanced on the road of evolution. In the case of Sirius and its faint companion, the mass of the bright star is about twice the mass of the satellite, while its light is about 40,000 times greater! Here the satellite is either a cooled down sun, or, possibly, a gaseous nebula. There seems to be no other explanation of this remarkable paradox. The same remarks apply to Procyon, where the bright star is about 100,000 times brighter than its faint companion, although its mass is only five times greater.

An apparent paradox is found in the case of the gaseous nebulæ. The undefined outlines of these objects render any attempt at measuring their distance very difficult, if not impossible. Their distance from the earth is, therefore, unknown, and likely to remain so for many years to come. It is possible that they may not be farther from us than some of the stars visible in their vicinity. On the other hand, they may lie far beyond them in space. But whatever may be their distance from the earth it may be easily shown

* I. Corinthians, Chap. 15, v. 41.

XUM

that their attraction on the sun is directly proportional to their distance; that is, the greater their distance the greater the attraction! This is evidently a paradox, and rather a startling one, too. But it is, nevertheless, mathematically true. *Their distance being unknown*, they may be of any dimensions. They might be comparatively small bodies relatively near the earth, or they might be vast masses of gas at an immense distance from us. The latter is, of course, the more probable. In either case the *apparent* size would be the same. Take the case of any round gaseous nebula. Assuming it to be of globular form, its *real* diameter will depend upon its distance from the earth. As the volumes of spheres vary as the cubes of their diameters, it follows that the volume of the nebula will vary as the cube of its distance from the earth. As the mass of an attracting body depends on its volume and density, its real mass will depend on the cube of its distance, the density, although unknown, being a fixed quantity. If at a certain distance its mass is M, it would, at double the distance (the *apparent* diameter being the same), have a mass of eight times M (eight being the cube of two), and at treble the distance its mass would be 27 M, and so on, its *apparent* size being known, but not its *real* size. This is obvious. Now the attractive power of a body varies directly as its mass; the greater the mass the greater the attraction. Again, the attraction varies *inversely* as the square of the distance, according to the well-known law of Newton. Hence, if d be the unknown distance of the nebula, we have its attractive power varying as d^3 divided by d^2, or directly as the distance d. We have thus the curious paradox that for a gaseous nebula whose distance is unknown its attractive power on the sun will vary directly as its distance, the greater the distance the greater the attraction, and, of course, conversely, the smaller the distance the less the attractive power. This result seems at first sight absurd and almost incredible, but a little consideration will show that it is quite correct. Consider a small wisp of cloud in our atmosphere. Its mass is almost infinitesimal, and its attraction on the earth practically *nil*. But a gaseous nebula having the same *apparent* size will have an enormous volume, and, although probably formed of a very tenuous gas, its mass will be very great, and its attractive power very considerable. The large apparent size of the Orion nebula shows that its volume is probably very great, and as its attraction on the sun is not appreciable, its density must be excessively small, probably less than the density of the air remaining in the receiver of the best air pump after the air has been exhausted. How such a tenuous gas can shine as it does forms another paradox. Its light is probably due to some phosphorescent or electrical action.

There is a similar paradox connected with the Milky Way. Taking any portion of it, its apparent area will be inversely proportional to the square of its distance from the earth, and as the light of each of its component stars is reduced in the same proportion, its light will remain constant whatever its distance may be. Hence the brightness of the Milky Way is no test of its distance, as some astronomers have erroneously supposed.

According to Newton's law the force of gravitation varies inversely as the square of the distance from the centre of force. This is found to be true in the solar system, and it also probably holds good in the binary stars. But this is not so in the revolutions of the stars composing a globular cluster round the centre of gravity of the cluster, which is probably situated near the centre of the sphere. From what Newton has proved about the internal attraction of a homogeneous sphere, it follows that in this case the force will vary *directly as the distance* from the centre, and each body composing the cluster (except those at the surface) will revolve round the centre of gravity of the whole, the period of revolution of each being the same.*

Another paradox is found in the motions of comets which approach very near the sun at their perihelion passage. Some have actually passed through the sun's corona, or outer envelope. This medium, although highly rarefied, produces a resistance to the motion of a comet in the same way that a meteor passing through the upper regions of the earth's atmosphere has its motion retarded; the lost energy being then converted into heat renders the body incandescent, and causes it to appear as a "shooting star." In the case of a comet one would think that the resisting medium would cause it to move slower, but the reverse of this is the case. The motion is certainly reduced at first, but owing to the diminished velocity the comet approaches nearer to the sun than it would otherwise do. Its orbit is thereby reduced in size, and the velocity is increased. This is a very curious fact, but is undoubtedly true. Encke's comet is an example. Its period (about 1,203 days) is shortening every revolution, and is now about 54 hours shorter than when it was discovered in 1819.

Many of the ordinary phenomena of the heavens are apparently true, although absolutely false, and these may, perhaps, be included among the paradoxes. The sun apparently moves round the earth from east to west in 24 hours, but its real motion is from west to east once in the course of a year. The apparent daily motion is, of course, due to the earth's real rotation on its axis in the opposite direction, and the apparent annual motion is caused by the earth's real revolution round the sun. The sun's apparent motion from east to west is evident to everyone, but its motion from west to east can only be detected by careful observation, and is probably unknown to many educated (or so-called "educated") people. By "careful observation" I do not mean observations with telescopes or astronomical instruments. I mean ordinary observation without any instrumental assistance. The simple fact that the stars set earlier every evening should be sufficient to show any intelligent observer that the sun has an apparent motion from west to east. This fact was known to the ancients 3,000 years ago, but in these days of "higher education" most people's eyes seem—like the man with "the Muck Rake"—to be so fixed on the baubles of earth that they are unable to see anything in the heavens. Similar remarks apply to the moon, but in this case the motion from west to east—which is here a *real* motion—is obvious, and is, perhaps, more generally recognised.

Another paradox is that the moon seems to be larger when near the horizon than it is when high in the sky. This is merely an optical illusion, for, as a matter of fact, the moon's apparent diameter is actually *less* when near the horizon, as theory requires, and careful measurement proves to be the case.

Many people speak of the earth as "a small place," because they happen to meet their friends at some spa in England, or some resort on the Continent, or elsewhere. But this idea is quite erroneous. Far from being small it is, compared with the size of a man or even a city, immensely large, or, we might even say, inconceivably gigantic. On the other hand, it is, compared with the size of the solar system, very small in-

* Newton's *Principia*, Book I., Prop. 89.

deed, and with reference to the Milky Way merely as a grain of sand on the ocean shore. It is, therefore, very large and very small at the same time; very large compared with the objects around us, and very small indeed in comparison with the vast extent of the visible universe.

Another curious paradox is that one can discover an object without seeing it, and see an object without discovering it. The planet Neptune was discovered by Adams and Leverrier by calculations before it was actually seen in the telescope by Galle; and it was actually seen by Lalande on May 8 and 10, 1795, but he took it for a star and thus missed the discovery. In fact, he *saw* the planet, but did not *discover* it. The great new star of 1901 was probably seen by some people before its discovery was announced, and it was actually noticed by a well-known American astronomer, who thought it was some bright star with which he was not familiar! But this did not amount to a discovery. Anyone absolutely ignorant of astronomy might have made the same observation. An object must be identified as new before a discovery can be claimed. Some years ago a well-known Dublin naturalist discovered a spider new to science, and after its discovery he found that it was common in nearly every house in Dublin! But this fact did not detract in the least from the merit of its scientific discovery.

Winter Cold in Relation to the Moon.

To the Editors of "Knowledge & Scientific News."

Sirs,—There is a feature of our winter cold (Greenwich) which it seems desirable to call attention to, as perhaps of some practical utility.

Confining our attention to four lunations, from December, it can be shown that *in each of the last seventeen winters*, the week about full moon (four cases) has always had more cold than the week about last quarter following (four cases).

We may proceed by counting the cold days in those weeks (meaning by a "cold day," one with mean temperature below the average for the day); or, going further, add the degrees of deficit on those cold days. By the former method, we find the full moon week to have more of cold days in each winter but two, which show equality; but on applying the other method to those two, the full moon week comes out distinctly the colder.

I have carried the inquiry back eight years, making 25, in all, and find two clear exceptions (winters ending 1884 and 1889), *i.e.*, week about last quarter the colder, and by both methods. And there may be others further back. But in any case, and apart from theory, it will be interesting to see how far this series of 17 winters is extended without break, or what exceptions occur.

Would it not be found generally, that, taking any group of, say, ten or more, consecutive winters (perhaps even five or more), the seven-day group showing most cold in the lunar cycle is centred somewhere from first quarter to full moon? (I write subject to correction.)

Having a copy of Luke Howard's work on the Climate of London, I lately noted the distribution of frost days in the 13 winters of which he furnishes data (1806-7 to 1818-19), taking four lunations as before. The coldest week was that ending first day after full moon (200 days), the warmest coming after new moon (162).

The above facts and speculation would appear to be in harmony with some popular impressions and folklore. There is, *e.g.*, the saying that "the full moon eats clouds" (in which case radiation would be favoured). Bacon remarks: "The weather is generally clearer at the full than at the other ages of the moon, but in winter the frost then is generally more intense."

Faithfully yours,

Alex. B. MacDOWALL.

The Age of Man.

One of the most promising subjects for discussion at the approaching meeting of the German Anthropological Association, is that of the natural or artificial origin of Eoliths. The terms of the Eolithic controversy were very fully stated in "Knowledge" last year ("The Eolithic Controversy," by J. Russel Larkby, March, 1906; and "Eolithic Man," by W. A. Dutt, April, 1906), and we need only recall the facts now briefly. The name Eolith was first proposed by Mr. J. Allen Brown* for specimens of alleged flint implements found on the summit of the North Downs by Mr. Benjamin Harrison, of Ightham, in Kent. These implements, if implements they are, are much ruder than the oval and leaf-shaped "Palæoliths" of the low-lying gravels of the Thames, Somme, and other rivers. They have, however, been found in gravels which are 500 feet above the level of the existing rivers; and consequently, according to Professor E. Ray Lankester, the Director of the Natural History Museum, to whose courtesy we are indebted for a photograph of eight Eoliths, forming part of the Prestwich collection, these implements, if really artificial, "carry the antiquity of man at least as far back beyond the Palæoliths as these are from the present day."†

Are Eoliths artifacts? This, to quote a recent address by Professor G. G. McCurdy,‡ is the fateful question. Their geological age is of no consequence if they are only natural forms and have never been used by man or his precursor. The first flint flakes to be utilised were in all probability natural forms. It is not likely that Eolithic man knew how to obtain the raw material from the chalk. He depended on picking up from the drift, flakes of approximately the shape and size needed. A sharp edge was utilised one, twice, or until it became dulled, and was then cast aside. If an angular piece did not admit of being comfortably grasped in the hand, the troublesome corners were removed. Such conclusions as these are forced on one after careful examination of a series of the specimens in question. Would the same conclusions be so irresistible if these objects were merely Nature's playthings? Many may even be grouped according to more or less definite patterns. Two of these deserve special mention; the small crescent-shaped scrapers comparable to the spokeshave, and the double scrapers with an intervening point between the two scraping edges.

These implements, in their form and susceptibility to classification, do more than suggest that they are not the outcome of chance operating by means of purely natural causes, because we can actually understand what primitive man wanted them for, and the primitive methods he used for adapting them to his needs. It is not sufficient then to say that the discovery of similar specimens in neighbourhoods and in circumstances where primitive man certainly had no hand in making them is sufficient proof of their natural origin. The chief argument against their artificial origin is what may be described as the Mantes Cement Factory argu-

* "Journal Anthropological Institute," March 8, 1892.

† "The Kingdom of Man," by E. Ray Lankester, D.Sc., F.R.S. (Illustration reproduced by kind permission of Archibald Constable and Co.)

‡ "Some Phases of Prehistoric Archaeology," *Science*, January 25, 1901.

XUM

ment*, which is the chief weapon in the hands of Professor Boule, M. Laville, and Herr Obermaier, the sceptics. The cement factory at Mantes deals with flints in the course of its business, and incidentally subjects a number of flint nodules to a highly disruptive action in revolving vats. It is suggested that the vat-whirlpool is not dissimilar in action from the vigorous eddies of pot holes in rivers. Therefore, if the vats

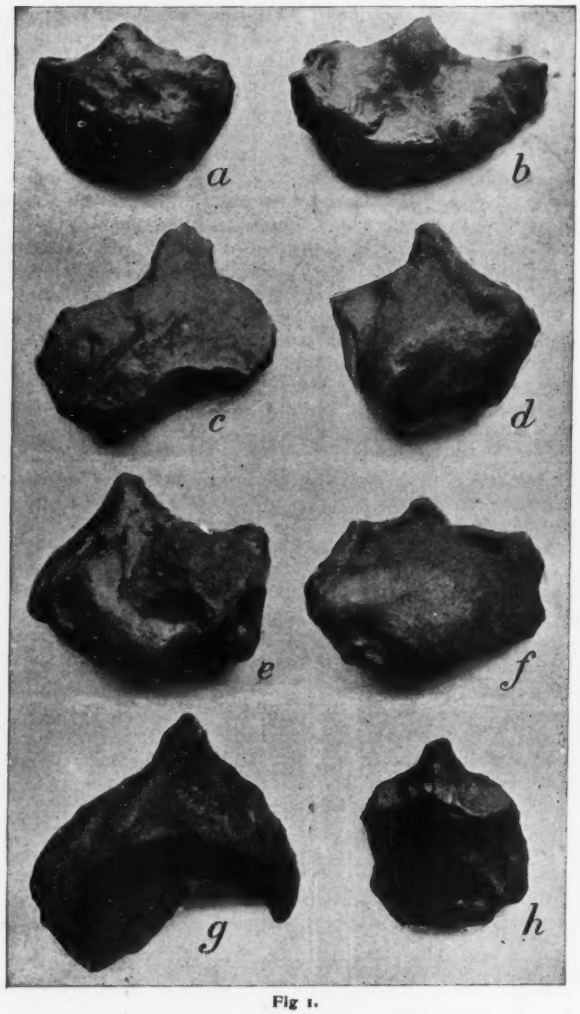


Fig 1.

Photograph of eight Eoliths of one and the same shape, namely, with a chipped or worked tooth-like prominence, rendering the flint fit for use as a "borer"—photographed to half the size (linear measurement) from specimens found near Ightham, Kent, in the high level gravel—which form part of the Prestwich Collection in the Natural History Museum, Cromwell Road. Many others of the same shape have been found in the same locality. These are far older than the oval, leaf-shaped "Palæoliths" of the low-lying gravels of the valleys of the Thames, Somme, and other rivers. (Reproduced by permission of Professor W. Ray Lankester, from "The Kingdom of Man." Constable.)

produce pieces of flint which are not unlike Eoliths, is it not probable, ask the sceptics, that the vigorous action of running water in forgotten river beds produced the Eoliths? The argument seems more cogent than it is. Nothing is really gained even by proving the impossibility of distinguishing between man's work and chipping due to natural causes. If the argument is worth anything it will admit of a still wider application, because of the fact that it is impossible to distinguish between certain true Eoliths and some artifacts of the Palæolithic and Neolithic periods. Things that are equal to the same thing are equal to one another. Therefore by substitution of the terms of the antithesis, one arrives at the identity between stream-made and machine-made Eoliths on the one hand, and recognised artifacts on the other. This does not prove the non-existence of true Eoliths any more than it proves the non-existence of Palæolithic or Neolithic implements. Moreover, if streams at flood ever produced Eoliths, it is more than probable that they are still doing so. Therefore, while keeping one eye on the cement mill refuse, the sceptics might do well to keep the other on the river. A few river-made Eoliths would certainly be more convincing than those turned out at the factory*.

The more widely distributed we find Eoliths of varieties susceptible to exact classification, the greater grows the probability of their artificial character. As most of our readers will be aware, the Kentish Eoliths find exact counterparts in the river terraces of Belgium; and in Northern Germany in the valleys of the Elbe and the Spree, and on the Island of Rügen. The chief contributors have been Professors H. Klaatsch, Eugene Bracht, and Max Verworn, and Drs. Hahne and Schweinfurth. Dr. Schweinfurth has carried out investigations in Egypt. English Eoliths are by no means confined to Kent. Berkshire, Dorset, Essex, Norfolk, and Suffolk (where, by the way, there is at Brandon a modern flint-chopping industry), and Wiltshire have all yielded specimens.

South Africa has also yielded examples of flint implements certainly older than the Palæolithic. A valuable survey of the problem, so far as South Africa is concerned, is given in "The Stone Implements of South Africa" (Longmans, Green and Co.), by Mr. J. P. Johnson. Mr. Johnson remarks in his preface that the terms Eolithic, Palæolithic, and Neolithic are frequently used in senses that are far from uniform. Stone implements show a progressive evolution from very primitive to very advanced forms, and the terms in question are usually expressive of certain stages in that progress. He applies the terms to the stone implements discovered in South Africa, only in so far as these implements are analogous to the groups of implements similarly labelled in England, and found respectively in the plateau gravels (Eolithic), valley drifts (Palæolithic), and alluvial flats (Neolithic) of the South of England. In Africa he has as far as possible adopted the rule of river terraces in assigning periods to the flint implements that have been found; that is to say, classifying the flint implements found on the highest terraces, or on the hill gravels, as the oldest. At Leijfontein, for example, where the best of his Eoliths have been found, there are patches of gravel which consist mainly of fragments of jasper—a material that must have been brought by water from a locality at least thirty miles away. The traces of the water, other than the gravel, no longer exist, so that the gravel beds must be of very great antiquity. Mixed with this gravel are quantities of much-worn and highly-polished Eoliths. A few are a little more advanced than the true Eoliths, but otherwise the group is in every way

* "KNOWLEDGE," April, 1906. See Photograph.

* A selected series of the Mantes cement factory flints was sent to the last meeting of the German Anthropological Association; and were afterwards examined by Professor Verworn and Dr. Hahne. Both these, as well as Dr. Rutot, who collects Eoliths in Belgium, are agreed as to radical differences between true and false Eoliths.

identical with the typical assemblage met with in the early plateau drifts of Southern England.

Mr. J. P. Johnson's first figure (Leijfontein series) shows a series of straight, concave, and convex-edged

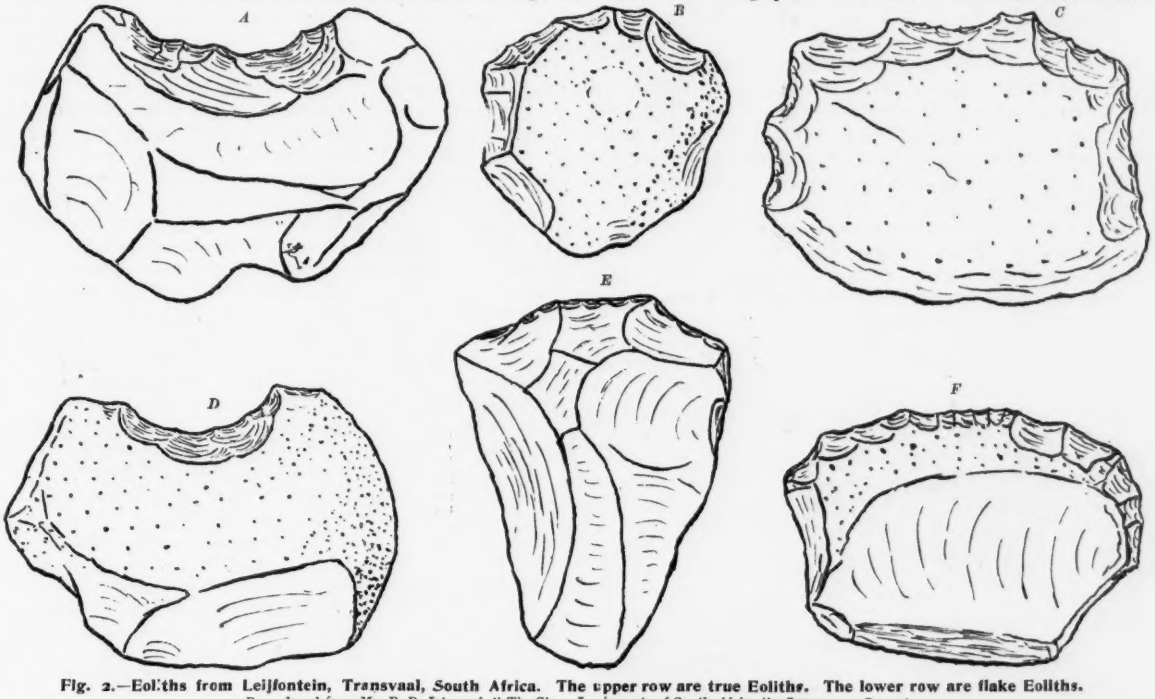


Fig. 2.—Eoliths from Leijfontein, Transvaal, South Africa. The upper row are true Eoliths. The lower row are flake Eoliths.
(Reproduced from Mr. P. R. Johnson's "The Stone Implements of South Africa." Longmans, Green.)

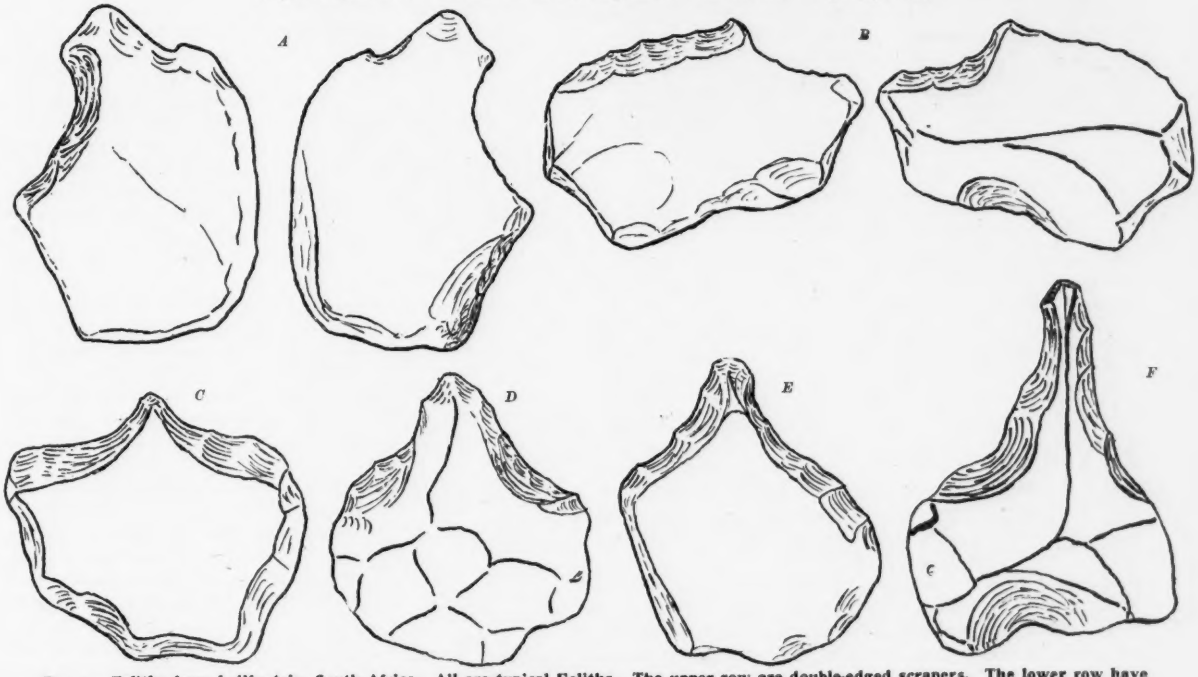


Fig. 3.—Eoliths from Leijfontein, South Africa. All are typical Eoliths. The upper row are double-edged scrapers. The lower row have both edges chipped on the same side.
(Reproduced from Mr. P. R. Johnson's "The Stone Implements of South Africa.")

scrapers. A, B, and C are true Eoliths, while D, E, and F are flake Eoliths. A and D are good examples of the concave scrapers. It will be noticed that there is quite a wide difference in the quality of the workmanship of these two. (There is a still greater difference between the better of these and Neolithic scrapers, and there are South African Palæolithic specimens which fill the gap.) But there is no essential difference between the disputed Eolithic examples and the accepted Neolithic ones. C, D, E, and F are four commonly recurring varieties of scraper. All of these can be matched by Palæolithic and Neolithic examples,

while one of them is still to be counted among the domestic appliances of certain savage peoples.

Mr. Johnson's second figure shows an extremely interesting series of implements. They are very typical of the Eolithic stage of culture, being rarely met with in more advanced assemblages. They are probably all scrapers. A and B are double-edged scrapers. It will be noticed that the chipping of the one edge is in the reverse direction to that of the other. The same characteristic is to be noted in English, Belgian, and French specimens. After chipping one of the margins, the Eolithic man, instead of rotating the specimen until the adjacent margin came into play, reversed it. They are an eloquent testimony to the artificial character of the Eoliths. It is incredible that a long, tapering point like that of specimen F could be hacked out by blind agencies, and the unlikelihood becomes greater with the multiplication of exactly similar examples.

We also reproduce a drawing of some flint implements which Major-General R. S. S. Baden-Powell

Fig. 4.—Stone Implements from the Zambesi.
Reproduced from Major-General R. S. S. Baden-Powell's "Sketches in Mafeking and East Africa." (Smith, Elder & Co.)

brought home with him from the Zambesi, the general agreement of which in form with certain types of scrapers will be perceived.

We cannot leave this subject without stating the strongest argument on the other side—the case against Eoliths. This is the instance of shore-made Eoliths, which Rutot and Hahne are now investigating on the Island of Rügen, in the Baltic. Flint nodules are here subjected to the violent action of the waves against the rocks; and the resulting fragments when secured at the proper time resemble more or less the true Eolith. They certainly form a more convincing argument than do those from the cement mills. But they resemble the cement mill Eolith more than they do the genuine Eolith, which, according to Hahne and Rutot, is still unaccounted for, unless it represents the handiwork of some intelligent animal, man, or his precursor. We may add in conclusion, that the belief in Eoliths commands continually more adherents, and, in the words of Professor Ray Lankester, the specimens which have been subjected most closely to examination "are too identical in shape, and so rationally of obvious utility, to permit of the possibility of their having been the result of fortuitous natural collisions." E. S. G.

Why Homogenised Milk Does Not Cream.

By J. C. GREGORY, B.Sc., A.I.C.

ON one occasion a certain lady was much exercised about the quality of the milk with which she was being supplied, because it did not appear to form sufficient cream on standing over-night. She changed her milkman, and was supplied with milk that gave a thicker layer of cream on standing.

Being interested in knowing what the actual difference between the supplies was, she had a sample of each analysed. The two samples were found to contain practically identical amounts of cream. This result showed in a striking way that the amount of "creaming" is not necessarily (though it often is) a test of the actual amount of cream present. So much, indeed, is this the case, that it is now possible to obtain milk which practically forms no layer of cream on standing. Such milk is now extensively sold as "homogenised milk." The term "homogenised," of course, refers to the cream remaining mixed throughout the body of the milk—forming a homogeneous liquid, and not separating into a separate upper layer.

To understand why milks containing equal amounts of cream may produce very different thicknesses of cream layers on standing it is necessary to remember exactly what cream is. When a portion of cream is examined through the microscope it is seen to consist of a number of shining globules. These globules are globules of fat. If a sample of milk is shaken up these fat globules are scattered all through the liquid. If the milk is now allowed to stand, unless it has been homogenised, some of the fat globules slowly rise to the surface to form a layer of cream. As has just been seen, it does not follow that this layer will contain all the fat globules—in the case of the homogenised milk it will contain none or very few. Since two milks actually containing the same amount of cream (that is of fat) may form different quantities of cream layers on standing for a certain time, it is evident that all fat globules do not rise with equal rapidity.

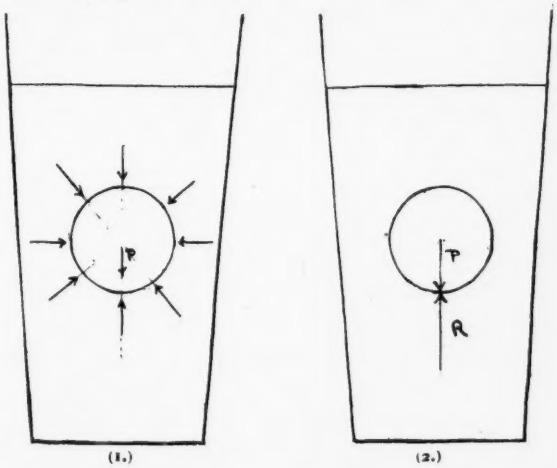
Now when the fat globules in cream are observed through the microscope, it is usually easy to notice that they differ in size. It is with this variation in size that the different rates at which they rise in the milk is closely connected.

In discussing this matter it will be convenient to consider milk as water containing a number of small fat globules. Average milk contains about three-and-a-half parts of fat in eighty-eight and-a-half parts of water. Other substances (casein, etc.) are, of course, also present, but for present purposes can be ignored. The fat globules are suspended in a fine state of division in the milk, forming an emulsion. The opaque whiteness of milk is very largely due to these finely divided, suspended particles. The gradual rising of some of these suspended fat globules produces "creaming." To understand why they should rise at different rates, it is necessary to grasp clearly the circumstances that make some bodies rise through or float in water.

Diagram 1 represents some water at rest in an ordinary tumbler. A portion of this water may be considered as forming a large globule suspended in the rest of the liquid. This globule is represented by the circle. Now this globule has weight and tends to fall to the bottom of the tumbler. Its weight is represented by the arrow-headed line P. Since, however, it is actually at rest, its weight must be counter-

balanced by another force. The eight arrow-headed lines outside the globule represent pressures exerted by the surrounding water upon it. An accurate representation would contain an infinite number of such arrow-headed lines, representing pressures, but the eight actually indicated are enough for the purpose. Since the net effect of all these pressures is to counterbalance the weight of the globule P, they must have the same effect as a single pressure, equal, and opposite to P. This single, counterbalancing pressure is indicated by the single arrow-headed line R in diagram 2.

Now suppose the circle to represent, not a globule of water, but a globule of lead. Then, since lead is heavier than water, P is greater than R, and the lead sinks. If the globule is cork, P is less than R, since cork is lighter than water, so that R pushes the cork up through the water and the cork ultimately floats on the top. In the same way if the globule is composed of milk fat (which is lighter than water) the water tends to steadily push it upwards.



Thus the fat globules rise through the water to form cream for the same reason that a cork rises up through it, viz., for the simple reason that volume for volume they are lighter than the water. But one characteristic feature of the rising of cream is its comparative slowness. It has been calculated that the largest fat globules of milk move upwards through it at the rate of rather over half an inch an hour. If a milk-jug contains milk whose surface is six inches from the bottom the largest fat globules at the very bottom of the jug will take just over ten hours to rise to the top. This rate contrasts very markedly with the rapid rise of a cork when it is held below the surface of water and then released.

On first thoughts there is a temptation to ascribe this comparative slowness to the fact that milk fat is heavier volume for volume than cork. To fix ideas, suppose that the imaginary globule of water represented by the circle in diagrams 1 and 2 weighs one ounce. Then P, the weight of the water globule, is a force equal to the weight of one ounce, and R has the same value. If the globule is cork it will only weigh about half an ounce. As before, R is equal to the weight of one ounce, but P is only equal to a half-ounce weight. The difference between these is half an ounce, and this represents the push upwards on the globule—itself weighing half an ounce. If the globule is milk fat it will weigh about nine-tenths of an ounce. In this case R equals one ounce and P nine-tenths of an ounce. Therefore the push upwards, which is the difference between these two, is about one-tenth of an ounce. The push upwards is less in the case of the fat than in the case of the cork, and the fat is itself heavier. The figures given are approximate, but are enough to show that fat globules will ascend less rapidly than cork globules.

But the reason just given is not sufficient to account for the very slow rise of the fat globules. A clue to a further reason is afforded by the observation that the smaller fat globules ascend less rapidly than the larger. It has been calculated that the smallest fat globules in milk travel upwards about 50 times as slowly as the largest. One of the smallest of the fat globules would, therefore, take about 20 days to rise from the bottom to the top of the milk in the jug previously mentioned.

It is evident, therefore, that the smaller the globule the greater the retardation to which it is subjected during its rise. This retardation is due to the resistance opposed by the water to the movements of the globules. This resistance is easily realised by pushing a flat stick in any direction through the water in a bath. It is also evident that the greater the surface presented by the body in proportion to its total volume the greater will be the proportionate resistance of the water. A piece of lead foil rolled up into a tight ball will tend to sink more rapidly than if it is beaten out into thin leaf. Now the less the size of a globule, the greater is the ratio between its surface and its volume. For example, consider two globules, one with a radius of two inches and the other with a radius of one inch. The larger globule will have a surface of just over 50 square inches and a volume of about $33\frac{1}{2}$ cubic inches. The smaller globule will have a surface of about $12\frac{1}{2}$ square inches and a volume of nearly $4\frac{1}{4}$ cubic inches. In the case of the larger globule the number representing its surface is less than twice as great as the number representing its volume, while in the case of the smaller it is nearly three times as great. In the case of exceedingly small particles the surface is very large compared with the volume. It is obvious that the more surface the globule presents relatively to its volume the more can the water oppose its motion.

Now the fat globules in milk are exceedingly small. A row of 15,875 of the smallest globules placed close together would measure an inch. In a similar row of the largest-sized globules there would be 2,540. The surfaces of these globules are so large in proportion to their volume (and, therefore, in proportion to their weight) that the water offers great resistance to their upward movement. This is the chief reason for the slowness of "creaming." A certain amount of cream is quickly formed by the particles of fat that happen to be near the top and have not far to travel, but the general process is slow.

The presence of the casein and other substances doubtless has an influence on the journeyings of the fat particles, but does not affect the general principles just indicated.

It is now evident that to produce a milk that will not cream the fat particles must be very finely divided by artificial means. This is actually done in "homogenised milk." The milk is squirted, at a high pressure, through very fine jets to break up the fat globules. The fat globules thus obtained are not usually larger than $\frac{1}{25000}$ of an inch in diameter. This state of division is so fine that under ordinary circumstances they are unable to force their way to the top of the milk to form a layer of cream.

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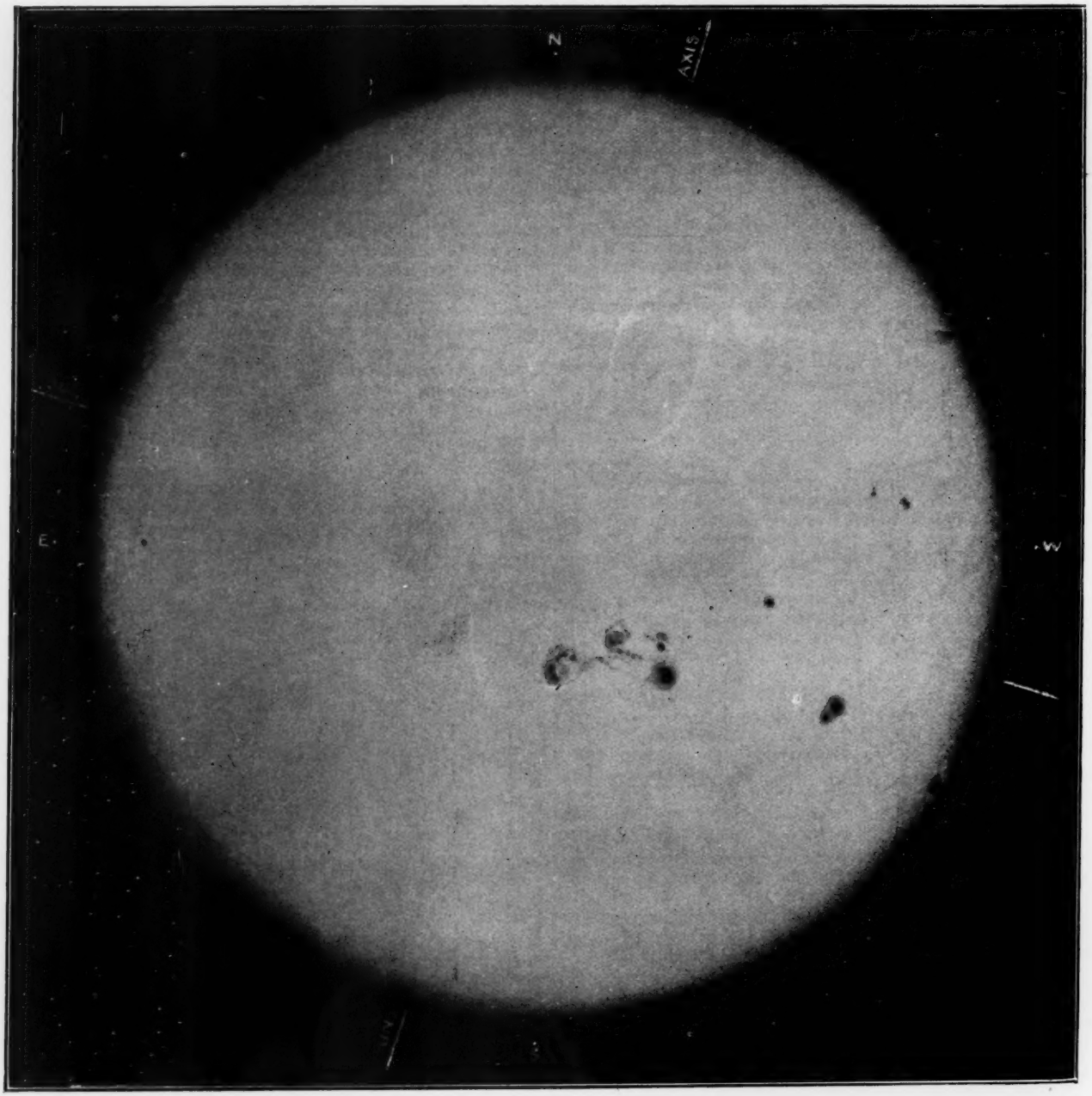


Photo. by E. W. Barlow, F.R.A.S.] **The Sun, February, 13, 1907, 10.1 a.m. Greenwich Civil Time.**

Sunspots, February 13, 1907.

By E. W. BARLOW, F.R.A.S.

The months of December, January, and February have been characterised by a very considerable increase in the activity of the solar surface, and February was especially noteworthy both for the number and extent of the sunspots visible. This month presented a great contrast to October last, during which a very few small spots were observed, the disc being entirely free from them on five days. The increase in activity was not confined to one region of the Sun, but the southern hemisphere has been chiefly affected by it. The accompanying photograph was taken at 10.1 a.m., on February 13, and the great group south and to the west of the centre of the disc formed one of the largest spots visible during the present maximum. It consisted of a large leader and a scattered rear-spot, with an intermediate extensive stream of a marked spiral form. The group extended for about 140,000 miles in length and 60,000 miles in breadth, and covered an area of nearly three thousand millions of square miles. The double spot further to the west was a return of a large group of spots visible from January 9 to January 21. On Saturday, February 9, when the large spot was crossing the central meridian, a great magnetic storm was recorded, beginning at 2.30 p.m., and lasting until 6 a.m. on Sunday. This storm was the most violent one experienced since that of October 31, 1903, and seriously affected telegraphic instruments. A very fine aurora was seen in the evening of the 9th over the greater portion of the British Isles. On February 10, four sunspots were visible to the unaided sight, a unique, if not unprecedented, occurrence. These were the two parts of the great group, the double spot west of it and the spot which is seen on the photograph just about to disappear round the west limb. The photograph was taken with a $4\frac{1}{2}''$ refractor. It is enlarged three diameters.

Physics and Biology.

By John Butler Burke, M.A. (Camb. and Dubl.).
Formerly Berkeley Fellow of Owens College, Manchester, &c.

(*Continued from page 62.*)

The more firmly we can rivet the idea in our minds that our place in the scale of being, like our nature, is a middle nature, the more ready shall we be to expect that phenomena, not unlike those in our own sphere, should take place on a smaller scale in the infinitely smaller worlds than ours.

Now I think this view cannot be too strongly emphasised. And I would discourage those who look for an explanation of the vast complexity of the phenomena of life we see around us, and of the still far greater complexity that must have been eliminated by natural selection, in the mere atomic properties with which the chemist is familiar. I would try to emphasise once more the inadequacy of such chemical units to account for the facts which are presented to us; and to impress the importance which such smaller and smaller units we have deemed it necessary to postulate in the infinite gradation must exercise upon the phenomenon of life in our own scale of being.

Recent work, and in fact, all work, on spontaneous generation would seem to come under the category of uncertainties. It is difficult to see how it could be otherwise; and how it can be proved that by any process of sterilisation in such experiments, the last traces of life have ever been annihilated. For although the organic life we are familiar with may be destroyed, some of the biophores or vital units of Weismann, ultra-microscopic forms, may, and most probably do, remain untouched or unaffected, like the immutable elements which compose the chemical constituents of the ever living protoplasmic substances themselves.

This everlasting controversy about that elusive and receding phantom called spontaneous generation, that disappears from our sight as soon as we may have thought that we had grasped it, owes its persistency, no doubt, to the difference of perspective, if we may put it so, in which the real point at issue has been viewed by those who have most eagerly pursued it.

Whether we are to regard Weismann's biophores or vital units as forming a continuous scale of ever simplifying vital forms *ad infinitum*—an aspect of the question which seems to be the most reasonable—or whether we should regard life as appearing from absolutely inanimate matter through mere chemical processes, by the artificial production of proteids, as Professor Fisher, of Berlin, has been trying to do, remains the one point at issue in the great problem of the origin of life.

Dr. Charlton Bastian has for thirty years or more affirmed again and again, against his colleagues in the world of science, that such a phenomenon does take place every day around us, and may be seen in the microscope to-day under our very nose. Whether he is right or not the future will decide. He appears to stand alone in the belief in spontaneous generation at the present day.

All the phenomena he has observed may be explained, I think, and yet not lose any of their importance, but perhaps have some importance added to them, on the supposition that they are the results of the growth of biophores in their development from ultra-microscopic forms.

In this germ-plasm, these biophores, we must seek the mystery of the origin of life. Weismann does not profess to go further. He is content to postulate that all the vast complexity of development of which these biophores admit results from the process inherent in them. But in order that this should be so, it is necessary that the physicist should step in and point out that there are difficulties from his point of view. Atoms and electrons, he would say, do not suffice him to explain the vast complexity of phenomena which these biophores require. And if the biologist were to press the point, as no doubt he would do, there is, I think, no alternative but that the atoms and electrons, which we have supposed to be the smallest, are in reality but aggregates of smaller things; whilst the assumption which is forced upon us affords, it is true, an opportunity to explain, not merely the evolution of the phenomena which are more properly biological, but the evolution of matter, electrical and chemical, from that self-same primordial substance.

We are forced, I think, from the assistance which biology thus lends us, to recognise in our own work as physicists, phenomena which that work would scarcely have led us to postulate.

In this, as in some other instances, it may be perceived that biology and physics overlap; and that, as in the case of geology, we may get a hint from the biologist that there is a residual phenomenon which we, as physicists, have yet ignored.

Still, it may be urged in passing, that the asymmetric structure can be explained, and has been explained, without any such hypothesis. True, but the invariable connection between it and living matter has not been. And in the "Origin of Life," my endeavour was to show that there is an element, a bio-element, possessing a vast store of potential properties, and of potential energy equivalent to biotic, so as to enable it to act as a nuclear substance, rotate the plane of polarisation, and though last, not least, admit of an almost indefinite variety of forms—as spiral nebulæ do—and yet be, from the chemist's point of view, like other elements. It would merely be the stage through which nebular biogen would have to pass in being transformed into carbon. And to that almost indefinite variety of biogen which the atoms would assume—or (should we say?) in the various stages of condensation—may be traced the indefinite potentialities of the biophoridæ. That indefinite variety cannot, as we say, be traced to their complex molecular structure, which are much too small to be composed of many atoms. Therefore it is that we seek to account for that indefinite variety by something else—in the difference of structure of the atom, the atom of the nuclear substance.

Professor Japp has found in the origin and behaviour of the tartrates one of the strongest reasons for his predilection towards biogenesis. In his famous presidential address to Section B, at the British Association, he emphasises how it is that physiologists have been carried away by Pasteur's subsequent work, in which the biological element predominates: and how they have neglected his earlier work. And yet he says: "It ought to be of interest to physiologists, not merely because it is the root from which the later work springs, but because it furnishes, he is convinced, a reply to the most fundamental question that physiology can propose to itself, namely, whether the phenomena of life are explicable in terms of chemistry and physics; in other words, whether they are reducible to problems of the kinetics of atoms; or whether, on the contrary, there are certain residual phenomena, inexplicable by such means, pointing to the existence of the directive force which enters upon the scene with life itself, and

XUM

which, whilst in no way violating the laws of the kinetics of atoms—though, indeed, acting through these laws—determines the course of their operation within the living organism."

And, again, if we view the matter from a physical standpoint, and from that standpoint alone, it does not appear, as Japp insists, that the explanations so far offered are from a physical point of view quite satisfactory. The statement that the carbon atom has a twisted structure conveys little to the mind of the physicist, unless the conception can be expressed in terms of mechanics, *i.e.*, traced to a dynamical foundation.

The origin of these structures of one-sided asymmetry is lost in the same obscurity as the origin of life itself.

Now this is the substance of which germ-plasm, or biophores, consist. Weismann, in his "Evolution Theory," translated by J. Arthur Thomson, Vol. II., p. 369, says: "In regard to them (biophores) alone is the possibility of origin through purely chemico-physical causes, without the co-operation of life already existing, admissible. It is only in regard to them that spontaneous generation is not inconceivable. We must, therefore, assume that at some time or other in the history of the earth, the conditions necessary to the development of these little invisible particles must have existed, and that the whole subsequent development of the organic world must have depended upon an aggregation of these biophores into larger complexes, and upon their differentiation within these complexes. We shall never be able, then, to observe spontaneous generation directly, for the simple reason that the smallest and lowest living particles which could arise through it, the biophoridæ, are so extremely far below the limits of visibility, that there is no hope of our ever being able to perceive them, even if we should succeed in producing them by spontaneous generation."

Dr. Windle, in commenting upon this passage in the *Dublin Review*, April, 1906, says he does not believe in Weismann's continuity of views. He compares his assumption within assumption, as he puts it, to the parenthesis within parenthesis, and re-parenthesis, which Mark Twain complains of in the "awful German language."

He pathetically, though we think scarcely logically, remarks: "It is assumed that the substance within the germ-cell is not simple, but complex. It is assumed that this complex body is made up of determinants for different parts of the body. It is assumed that these again are built up of vital units, each living its own life; struggling with its neighbours, influenced by the nutrition stream by which it is bathed; and finally, by an all-embracing assumption, these unseen, unprovable vital units, are erected into a new family of living things, the 'biophoridæ,' and we are told they were spontaneously generated, and that no man can prove the contrary, for they are, and must always be, invisible." Dr. Windle is, no doubt, aware that molecules, atoms, and electrons are equally invisible; and yet no man of science at the present day would question their existence. Visibility is not always the test of the reality of phenomena. If it were, the whole fabric of modern molecular physics would break down. It is the coherency of a theory, the fact that it can predict events which can afterwards be verified, which constitutes its value. Now, although we do not for a moment underrate the importance of Professor Windle's argument—for we know well that it is the inadequacy of the germ-plasm hypothesis, in its present form, and not the invisibility of biophoridæ, with which he is dissatisfied—we think that it is in this respect physics may move forward and lend a helping hand to the sister science of biology. When we come down to biophoridæ, we find ourselves at length in the region of molecular dimensions; and in the realm not merely of the chemist, but of the physicist as well. It is indispensable that if the discussion is to be continued, we should form an *entente cordiale*, for the biologist is no longer in undisputed territory. His resentment at the interference from without in matters which he has been accustomed to regard as exclusively his own concern, now awakens in him the sense that at last he has reached a foreign land, where the modes of thought and of expression are adequate in themselves, but not quite those with which he has been familiar. In this unconscious trespass the physicist, too, has much to learn from this welcome visitor. And it is that which it has been my object in this essay to point out.

Yet, as Dr. Windle imagines Weismann to say, "Give me my determinants and I will explain anything you can ask me." Certainly, and give me an evolutionary hypothesis of matter, such as that which I have sketched out, and I will show you that your determinants are a necessary consequence of it. There is nothing outrageous or improbable in this evolutionary conception of corpuscles and atoms from a primordial substance, whose elements are necessarily infinitely smaller still. This is what the idea of continuity would lead us to expect, *ad infinitum*. We acquire, I think, a clearer and more comprehensive grasp of the world, by thus looking upon phenomena as the result of a continuous graduation of events from the infinitely great to the infinitely small.

I know well there are many who will not enter into this frame of mind, or swallow this idea, if I may put it so. But there are others who can, and do. The fascination for that great idea, as I say, ever presses upon me the importance of thinking of the farthest limit we may reach to be a halting step towards that which may be reached. And the mystery of life lies in that untravelled country which no investigator has traversed, or will ever reach—the world of the infinitely small. Leibnitz, as we may remember, regarded his monads and atoms as points, infinitely small, and without extension in space, but extended in another dimension in the world of mind. This is the view of Nature which, as I have tried to show elsewhere, approaches or resembles somehow the theory of matter and mind-stuff which may be made to harmonise with our theory of life. In the monadology, and in the depth of intellect of that renowned philosopher's great conception, may we seek to grasp the meaning of that middle nature which is our being, in which we may most readily perceive the subjectivity of all scientific phenomena, and the basis of that consciousness which makes our knowledge possible. Biologists, with all due respect to them, will do well to recognise where the real difficulty lies, as it is to be perceived.

COUNT PLUNKETT, F.S.A., and Dr. Cochrane, C.E., have been asked by the Royal Society of Antiquaries of Ireland to represent the Society at the next International Congress of Prehistoric Anthropology and Archæology, which will be held in Dublin in 1909.

Photography: Pure and Applied.

By Chapman Jones, F.I.C., F.C.S., &c.

Electric Effects Produced by Light.

It has been repeatedly observed that if two similar metallic plates are immersed in a suitable liquid and then one of the plates is illuminated while the other remains dark, the light produces a disturbance at the illuminated plate and upsets the balance of the arrangement. The two plates become dissimilar, and therefore an electric current is produced in a wire that joins them. Becquerel and Minchin, in their investigations of such effects, found such irregularities in their results that they failed to establish any general principle, and supposed that there existed some obscure surface action, so that it appeared as if light in this case was not comparable to other forces, such as heat. Dr. M. Wilderman has recently been working at the same subject, under probably more stringent conditions than the previous investigators, and he finds that light and heat are strictly analogous, that irregularities previously noticed were due to polarization of the electrodes, or possibly sometimes to insufficient care in getting them perfectly balanced before the experiment. For however carefully two silver plates, cut from the same sheet of the purest metal obtainable, are annealed and polished, it may be necessary to keep them in the liquid for one or two days, or even more, before they are sufficiently alike to produce no electromotive force of the order of intensity observed in these experiments when both remain in the dark. When light, as from an acetylene flame or an electric arc, is allowed to impinge upon one plate of such a balanced system, an electromotive force is produced which is due partly to a rise of temperature, and partly to what Mr. Wilderman calls "light at a constant temperature." Of course, radiant energy of a given wave length is not supposed to be divisible into two parts of different characters, but it seems that the disturbance it causes shows itself partly as heat, which in turn produces an electromotive force, while part produces an electromotive force directly, without the intervention of heat. The word "light" is rather an unfortunate term in this connection, because, as commonly understood, it is an effect upon the optic nerve, and the limitations of the sensitiveness of our eyes does not enter into this matter at all. When a red screen was interposed in the path of the light, the resulting red light gave about three-fourths of the heating effect of the unscreened light, and only a very small proportion of that due to light at a constant temperature. In these changes, the amount of chemical change is so minute that it would take about thirty years to separate by decomposition a milligramme of silver. Such an arrangement as described constitutes a true galvanic cell, but it differs from ordinary galvanic cells in being a galvanic combination only so long as it is exposed to light, and in *gradually* developing its maximum electromotive force in the presence of light, and *gradually* losing it when the light is withdrawn.

I do not know that these experiments have any obviously direct bearing upon current photographic practice. But the very essence of photography consists in partially illuminating a surface that is intended to be of a uniform character. The light causes a change, and it seems that all changes are accompanied by electrical disturbances. These experiments may help to show something of the character of the electrical changes that accompany the effects produced by "exposing" sensitive surfaces.

The Use of the Fixing Bath.

Messrs. Lumière and Seyewetz have recently been estimating how many plates may be safely fixed in a certain bulk of fixing bath, and although their actual figures are, in my opinion, of neither theoretical nor practical interest, they have found some facts that are well worth notice. Their figures are not of practical interest because a hundred plates of a certain size fixed in a litre of a certain strength is not a definite statement. It is not the superficial area of plate, but the amount of silver salt that is important, and the two are not proportional. Nor is it well to use a fixing bath to a calculated extent. Unless a fresh portion is taken for every two or three plates, so that the solution is a very long way from the possibility of its being used too much, two portions should be used, fixing in the first until the plate is clear, and then soaking it for about an equal time in the second. When the first begins to work slowly it is discarded, the second solution takes its place, and a fresh portion is taken into use for the second.

The chief fact of interest that these experiments have led to is that the acid fixing bath used was found to have only half the fixing power of a plain solution of sodium thiosulphate before it becomes unsafe. This, therefore, emphatically supports the opinion that I have expressed for many years, that acid fixing and clearing baths are dangerous—tending to a want of permanence in the negative. It is curious, however, to note that the addition of a little chrome alum to the acid fixing bath increases its safe fixing power, and shows a loss of only twenty-five per cent. instead of fifty per cent. of usefulness. The effect of incomplete fixation may take months, if not years, to show itself, but if before it becomes obvious any change is needed in the negative, such as intensification, then the impure film gives an irregular result and perhaps a hopelessly stained negative.

The fixing bath employed in the above experiments was acidified by the simple addition of a little sodium bisulphite, which is perhaps the least objectionable of the various methods proposed for preparing such a solution.

Received.

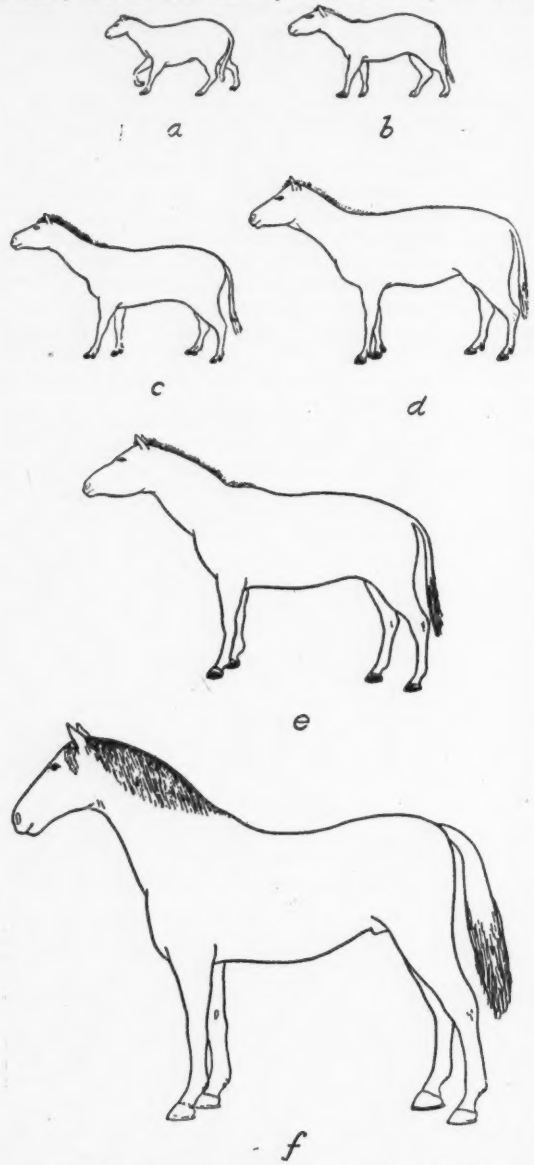
The "Euryplan" lens of Messrs. A. E. Staley and Co. is entitled to rank as a first-class anastigmat, although it is more moderate in price than many similar instruments. It is a symmetrical doublet. Each combination consists of three lenses with an air space, and may be used alone, or sets may be arranged of one mount and two or more combinations of different focal lengths. The mount is well made, and altogether, judging from an example that I have handled, these lenses well merit the praise that has been bestowed upon them.

Those interested in photo-micrography should get a little pamphlet from Messrs. Wratten and Wainwright that gives the spectrum absorptions of the stains generally used, with the colour-sensitive plate and colour-screens best suited for each, besides other information. The "Wratten M" screens, made for this purpose, are of nine varieties of colour, and the matching of the plate and colour-screen to the stain is made spectroscopically, the tables giving the wave-lengths of the light concerned.

XUM

The Ancestry of the Horse.

IN a note to the article on the "Ancestry of the Horse," which Mr. R. Lydekker, F.R.S., contributed to "KNOWLEDGE," in January, 1904, the author referred to the collection of fossil remains of the early ancestors of the modern horse, which is possessed by the United States; and to the claim put forward by American



Prehistoric horses compared with the modern horse in size and contour: *a*, *Protorohippus* (Wind River Eocene); *b*, *Orohippus* (Bridger Eocene); *c*, *Mesohippus* (Oligocene); *d*, *Merychippus* (Miocene); *e*, *Pliohippus* (Pliocene); *f*, *Equus* (Recent). (Original.)

palæontologists, that the early development of the horse took place in the Western hemisphere. Mr. Richard S. Lull has recently written, and the *American Journal of Science* has reproduced, an article on the "Evolution of the Horse Family," as illustrated in the Yale University collections. From it we quote the following extracts, and reproduce the accompanying plate.

"The completeness of the record of the evolution of the horse tells us something of the enormous numbers of ancestral forms which must have existed in the more than two million years that have elapsed since the first diminutive horse appeared in North America. While not strongly given to migration, in the course of time these animals wandered over the entire world, with the exception of such inaccessible places as Australia and the Oceanic Islands. . . . It would seem that the original stock was of Eurasian derivation, though the great theatre of the evolutionary drama was soon transferred to North America, the Eurasian, African, and South American horses which appear from time to time being in all probability of North American origin. The ultimate fate of the horses in both North and South America was extinction, all wild horses of our own time, including the asses and zebras, being confined to Asia and Africa. The apparently wild bands of the American western plains, and those which roam over the pampas of South America, are feral, which is to say, that they are the descendants of domestic horses that have escaped from human bondage, largely from the early Spanish explorers.

"The first undoubted horselike animal was *Eohippus*, a little creature, about eleven inches in height at the shoulder, and in general suggestive of the carnivores rather than of the ungulates of to-day. The back was arched, the head and neck were short, and the limbs of moderate length, showing no remarkable adaptation for speed. This genus has a remarkable geographical range, having apparently originated in England (then Western Europe), and migrated by way of Europe and Asia, and what is now Behring Straits, to America, where it got as far south-east as New Mexico. This migration of *Eohippus* shifted the scene of evolution to the Western hemisphere, for while the remains of succeeding genera are increasingly numerous in North American rocks, from the Wasatch onwards, it is only from time to time that European representatives appear, in each case evidently derived from migratory North American types. . . .

"During the succeeding Wind River Age, *Protorohippus* appears, representing the next stage in the evolution of the horse. In stature somewhat larger than *Eohippus*, this animal reached the height of fourteen inches, and while the beginnings of speed are evident, the bodily contour is still primitive. *Orohippus* also belongs to a later stage of the Eocene."

Subsequent developments of the horse and the periods to which they belong are indicated in the illustration which we reproduce. *Equus*, the modern horse, first appears in the Upper Pliocene Beds of Europe, Asia, and North America, and represents the culmination of the race.

Lewis' Circulating Scientific Library.

WE have received a copy of the last "Quarterly List" of additions to this well-known library, and find that all the important works in general science, as well as those dealing with medical and surgical subjects, have been added to the shelves for the use of subscribers. We observe that the rapid growth of the library during recent years has necessitated an extension of premises, which permits of every convenience being available to subscribers for the personal use of the library, such as for making extracts and writing.

The Weather and Influenza.

By Dr. J. G. McPherson, F.R.S.E.
(*Late Extension Lecturer on Meteorology in the University of St. Andrews.*)

This insidious disease of influenza shows no respect of persons. All classes and ages are its victims. And its dregs are frequently fraught with lasting danger.

Some remarkable facts connecting influenza and the weather have been deduced by the late Dr. L. Gillespie, Medical Registrar, from the records of the Royal Society of Edinburgh, now before me. He considered that it might lead to interesting results if the admissions into the medical wards were contrasted with the varying states of the atmosphere. The repeated attacks of influenza made him pay particular attention to the influence of the temperature and rainfall on that disease.

The meteorological facts taken comprise the weekly type of weather, *i.e.*, cyclonic or anti-cyclonic, the extremes of temperature for the district for each week, and the mean weekly rainfall for the same district. More use is made of the extremes than of the mean, for rapid changes of temperature have a greater influence on disease than the actual mean.

The period which he took up comprises the seven years 1888-1895. There was a yearly average of admissions of 3,938; so that he had a good field for observation. Six distinct epidemics of influenza, varying in intensity, occurred during that period; yet there had been only 23 attacks between 1510 and 1890. Accordingly, these six epidemics must have had a great influence on the incidence of disease in the same period, knowing the vigorous action of the poison on the respiratory, the circulatory, and the nervous systems. The epidemics of influenza recorded in this country have usually occurred during the winter months.

The first epidemic, which began on December 15, 1889, and continued for nine weeks, was preceded by six weeks of cyclonic weather, which was not, however, accompanied by a heavy rainfall. Throughout the course of the disease the type continued to be almost exclusively cyclonic, with a heavy rainfall, a high temperature, and a great deficiency of sunshine. The four weeks immediately following were also chiefly cyclonic, but with a smaller rainfall.

The summer epidemic of 1891 followed a fine winter and spring, during which anti-cyclonic conditions were largely prevalent. But the epidemic was immediately preceded by wet weather and a low barometer. It took place in dry weather, and was followed by wet, cyclonic weather in turn.

The great winter epidemic of 1891 followed an extremely wet and broken autumn. Simultaneously with the establishment of an anti-cyclone, with east wind, practically no rain, and a lowering temperature, the influenza commenced. Great extremes in the temperature followed, the advent of warmer weather and more equable days witnessing the disappearance of the disease.

The fourth epidemic was preceded by a wet period, ushered in by dry weather, accompanied by great heat; and its close occurred in slightly wetter weather, but under anti-cyclonic conditions.

The fifth outbreak began after a short anti-cyclone had become established over our islands, continued during a long spell of cyclonic weather, with a considerable rainfall, but was drowned out by heavy rains.

The last appearance of the modern plague, of which Dr. Gillespie's paper treats, commenced after cold and wet weather, continued in very cold but drier weather, and subsided in warmth with a moderate rainfall.

The conditions of these six epidemics were very variable in some respects, and regular in others. The most constant condition was the decreased rainfall at the time when the disease was becoming epidemic. Anti-cyclonic weather prevailed at the time.

According to Dr. Gillespie, the tables seem to suggest that a type of weather, which is liable to cause catarrhs and other affections of the respiratory tract, precedes the attacks of influenza; but that the occurrence of influenza in *epidemic form* does not appear to take place until another and drier type has been established. As the weather changes, the affected patients increase with a rush.

Dr. Gillespie came to the conclusion that the supposed rapid spread of influenza or the establishment of anti-cyclonic conditions may be explained in this way. The air in the cyclonic vortex, drawn chiefly from the atmosphere over the ocean, is moist, and contains none of the contagion; the air of the anti-cyclone, derived from the higher strata, and thus from distant cyclones, descending, blows gently over the land to the nearest cyclone; and, being drier, is more able to carry suspended particles with it. He formed the opinion that temperature has nothing to do with the problem, except in so far as the different types of weather may modify it.

The Infirmary Records point to the occurrence of similar phenomena, recorded on previous occasions. Accordingly, if such meteorological conditions are not indispensable to the spread of influenza in epidemic form, they at least afford favourable facilities for it.

Practical Aerodynamics And the Theory of Aeroplanes.

By Major B. Baden-Powell.

III.

While discussing the subject of the exact pressure developed by a current of air at a given speed, it is worth while again laying stress on the extraordinary fact that till quite recently all tables of wind pressures, on which our engineers and architects have based their calculations for buildings and structures, have been copied word for word and figure for figure from the old table compiled by Mr. Rouse over 150 years ago. Nor does there seem to be any record of exactly how the figures were arrived at. Smeaton, in his paper on "The Natural Powers of Water and Wind" (*Phil. Trans.* Vol. LI., 1759), merely says:—

"Some years ago Mr. Rouse, an ingenious gentleman of Harborough, in Leicestershire, set about trying experiments on the velocity of the wind and force thereof upon plane surfaces and windmill sails; and much about the same time Mr. Ellicott contrived a machine for the use of the late celebrated Mr. B. Robins for trying the resistance of plane surfaces moving through the air. The machines of both these gentlemen were much alike, though at that time totally unacquainted with each other's inquiries." And later in the same paper he quotes the table, prefacing it with these remarks: "The following table, which was communicated to me by my friend, Mr. Rouse, and which appears to have been constructed with great care from a considerable number of facts and experiments."

As almost all the more recent practical tests, which have been made with all care and with the best appliances of modern science, have shown pressures con-

XUM

siderably less than those in this table, it is not unreasonable to assume that Rouse's figures are incorrect. But now another fact must be referred to.

A number of writers on the subject have endeavoured to get at a theoretical basis for ascertaining the pressure. It has been laid down (Rankine, &c.) as a law of fluids that the magnitude of the pressure of the stream against any surface bears to the weight of the fluid the same ratio that the velocity of the current bears to the velocity generated by gravity. That is to say, for air, the pressure is to the weight of a cubic foot of air (.076 lb.) in the same proportion as velocity is to 32 feet per second. Or, working out the equation, $P = \frac{.076\ v}{32.2} = .00237$ v. in feet per second, or just about .005 miles per hour. It is, therefore, noteworthy that this theoretical pressure gives exactly Rouse's equivalent for K.

This theoretical computation does not, however, take into account the conical cushion of air which is formed in front of the plane, or other factors which tend to lessen this pressure.

It is evident that in computing the pressure of air, due allowance must be made for the density of the air, but this will be dependent both on the temperature and barometic pressure. When the air is warm and expanded the pressure it is capable of exerting is bound to be less than when it is cold and dense.

The following formula is given by Kempe (*Engineer's Year Book*) to get the velocity of wind in feet per second, when the air moves from a state of greater density H to one of less density h, t being the temperature :—

$$v = 1347.4\sqrt{\frac{H-h}{h}}(1+0.02088\ t.)$$

In 1876, Mr. A. R. Wolff* published, in New York, a theoretical method of determining the pressure corresponding to a given velocity of wind. He compiled a complete table giving velocities in miles per hour and pressures at temperatures from 0° to 100° F. This was based on the theoretical value of K at .005.

A number of deductions have since been come to by various experimenters in different countries, which may be briefly summarised.

Capt. Thibault, in 1826, made various experiments on the resistance of plane surfaces, and found $K = 0.00475\ v^2$.

In 1869, Capt. Murray, R.N., wrote a letter to the Aëronautical Society of Great Britain in which he said : "As at present the Society and its members are groping in the dark for want of any knowledge of the fundamental relation between pressure and velocity of air, they should offer a substantial prize—of not less than £100—for a complete and general solution, on rigid mathematical principles, of the following problem : 'Required, the relation between the velocity of a current of air and its pressure on a plane surface of given size, shape, and inclination.'" The result of this communication was that a subscription was raised, apparatus was constructed by Mr. Browning, and experimented with by Mr. Wenham in 1871. But as these were chiefly directed towards obtaining data on inclined surfaces, we will refer to them again later on.

Hagen, in Berlin, in 1874, made a series of careful observations on wind pressures, which gave materially different results to those formerly adopted. His value for K was .00359.

According to the experiments of Du Buat, K would equal .004864.

In 1876, Lord Rayleigh deducted theoretically a value of .00225.

Ritter von Loessl, experimenting in Vienna in 1881, found greater resistances, K working out at about .00512.

Colonel Renard, who conducted numerous experiments at the military establishment at Chalais-Meudon, as well as on the Eiffel Tower, made K about .0035.

Probably the most extensive and careful experiments which have been undertaken in this line were those conducted by Prof. S. P. Langley* in 1888-90 at the Smithsonian Institution. These must frequently be referred to hereafter, as they embraced a number of tests made with different forms of apparatus. The "Rolling carriage" consisted of a plane surface mounted on the end of a horizontal brass tube which was suspended from wheels running on a fixed bar. A spiral spring held the tube forward until pushed back by the pressure of the air on the plane. The whole instrument was mounted on the end of the arm of a large whirling table 60 feet in diameter. This showed a pressure, varying with different conditions, of .00389 to .00326 v^2.

In 1889 Mr. W. H. Dines made a series of experiments with a large whirling table, 56 feet in diameter, at Hersham.† This was rotated at speeds up to 40 miles an hour, and the pressures were measured on a plate of polished wood one foot square. The results showed that the pressure was .0029 v^2, or 1 lb. per square foot at 18½ miles per hour.

In 1899 M. l'Abbé Le Dantic, in collaboration with M. Canovetti, conducted some experiments for the *Société d'Encouragement pour l'Industrie Nationale*, which body had offered a prize for investigations on "the reactions on a surface moving through the air under varying conditions as to forms and velocity."

In order to get a large space quite free from air currents the tests were conducted in the Chapel of the Conservatoire des Arts et Métiers. The surface to be experimented on was caused to slide down a vertical wire. The weight and area of the surface being accurately known, the time of fall was observed by electric recording apparatus. A plane surface of one square metre moving with a velocity of one metre per second was found to experience a resistance of 81 grammes. This would work out as K = .0032.

M. Canovetti‡ also carried out a number of experiments on a large scale at Brescia. He attached the various surfaces and models to be experimented upon on a light carriage arranged to run down an inclined wire which was stretched from the top of some fortifications to the plain 70 metres below. The wire was 380 metres long. Later he made further trials on a truck running down an inclined railway. Though he obtained numerous results with different shapes and sizes of bodies, we need here only mention that his values for K varied from .0026 to .00426. He, unlike some other authorities, considered that the proportional resistance decreased with the speed. Thus, with a rectangle of 1.85 square metre with a speed of 5.64 m. K (in metrical figures) = .09, while travelling at 7.09 m. it would equal .084.

Dr. Stanton‖ of the National Physical Laboratory,

* "The Windmill as a Prime Mover," by A. R. Wolff, New York, 1885.

* "Experiments in Aerodynamics," by S. P. Langley, Washington. 1891.

† Quarterly Journal of the Royal Meteorological Soc., Oct., 1889.

‡ "Rapport sur les Expériences Rélatives à la Résistance opposée par l'Air," Soc. d'Encouragement pour l'Industrie, Paris, 1903.

‖ "On the Resistance of Plane Surfaces in a Current of Air," by T. E. Stanton, D.Sc. Reprint from the *Proceedings of the Inst. of Civil Engineers.* Vol. CLVI, 1904.

undertook some experiments in 1903. From these he deduced a still lower value for K, viz., .0027.

By taking a general average of these results (which evidently vary according to the size, shape, speed, barometric pressure, and other conditions) we shall not be far wrong in assuming a value for K of .003. A general table for practical use may then be made out, as follows :—

Table of Air Pressures.

$P = .003\ v^2$

Miles per Hour.	Feet per Second.	Lbs. per Square Foot.	Miles per Hour.	Feet per Second.	Lbs. per Square Foot.
1 ..	1·47 ..	·003	19 ..	27·88 ..	1·083
2 ..	2·93 ..	·012	20 ..	29 35 ..	1·200
3 ..	4·41 ..	·027	25 ..	36·70 ..	1·875
4 ..	5·87 ..	·048	30 ..	43·98 ..	2·700
5 ..	7·33 ..	·075	35 ..	51·32 ..	3·675
6 ..	8·87 ..	·108	40 ..	58·68 ..	4·800
7 ..	10·29 ..	·147	45 ..	66·01 ..	6 075
8 ..	11·76 ..	·192	50 ..	73·34 ..	7·500
9 ..	13·23 ..	·243	55 ..	80·67 ..	9·075
10 ..	14·66 ..	·300	60 ..	87·95 ..	10·800
11 ..	16·12 ..	·363	65 ..	95·29 ..	12·675
12 ..	17·59 ..	·432	70 ..	102·62 ..	14 700
13 ..	19·06 ..	·507	75 ..	109·95 ..	16·875
14 ..	20·53 ..	·588	80 ..	117·28 ..	19·200
15 ..	22·00 ..	·675	85 ..	124·61 ..	21·675
16 ..	23·47 ..	·768	90 ..	131·94 ..	24·300
17 ..	24·94 ..	·867	95 ..	139·27 ..	27·075
18 ..	26·41 ..	·972	100 ..	146·60 ..	30·000

(*To be continued.*)

ASTRONOMICAL.

By Charles P. Butler, A.R.C.Sc. (Lond.), F.R.P.S

New Form of Coelostat Telescope.

Professor G. E. Hale has recently published an interesting account of a new form of coelostat telescope which he proposes to erect at the Mount Wilson Observatory, the design being arranged to eliminate as far as possible the difficulties encountered in recent work. These have been due chiefly to distortion of the mirrors and local disturbances of the atmosphere caused by the sun's heat, all of which are more serious than are encountered during the course of night observations.

An approach to an ideal equipment is outlined, including a coelostat (to insure there being no rotation of the image), mounted at a considerable height above the ground, to reduce the effect of the hot air rising from the ground. The ground about the instrument should be shaded for this same reason. It will be best to arrange the instrument for early morning and late afternoon observations, the former being the most important, because of the better definition at that time. All the mirrors employed should be completely filled with sunlight to avoid irregular distortion, and should be extremely thick, to reduce the amount of bending by the heat of the sun's rays. The beam of light should be vertical, and the image formed by an object-glass rather than a concave mirror. The spectroscopic equipment to be below ground, and all precautions taken to keep the temperature as constant as possible. In carrying out this programme two galvanised steel towers about 60 feet high are to be erected over the mouth of a pit some 30 feet deep. The central tower will carry the coelostat, secondary mirror, and object-glass of 60 feet focal length, and the outer tower is to be encased with canvas louvres to protect the inner one from wind. The mirrors (17 inches diameter and 12 inches thick) are to be silvered on both surfaces, the back being heated by sunlight reflected from other mirrors. From these the light is reflected to the object-glass of 12 inches diameter, giving an image of the sun about 6 inches diameter on the slit plate of the spectroheliograph or Littrow spectroscope below. These instruments are to be provided with objectives of 30 feet focal length. To produce the requisite translatory motion of the solar image the object-glass will be traversed by an electro-motor, which, by suitable gearing, also moves the plate-holder outside the secondary slit, passing K or other monochromatic radiation to the photographic plate. Three slits are to be employed, so that three photographs of the same part of the sun may be taken simultaneously with the aid of three different lines, the comparison of which should prove extremely interesting and instructive.

The Littrow spectrograph will contain a plane grating of eight inches aperture, having 500 to the millimetre, and will be employed for the photographic study of the solar rotation and sunspot spectra.

Orbit of Spectroscopic Binary λ Andromedae.

Campbell announced the binary character of this star in 1899, and from photographs taken during that year its period was determined to be 20.5 days. Some of these plates were taken with the original Mills spectrograph, and others with the altered instrument. Definitive measurements appeared to show a discrepancy between the two series, so a complete series was secured with the newer instrument in order to determine the nature of the agreement. From these new plates series of elements for the star system were computed. If the period be changed to 20.523 days and a constant correction applied to one series, the observations can be reconciled, but as the spectrum lines are so well adapted for measurement it is considered that this should be unnecessary, and that the change in elements may be real. A series of plates taken after the lapse of a few years would no doubt establish or disprove the reality of such change, and such a series should be taken in a single cycle of 20.5 days if possible, in order to eliminate the effects of changing elements and error in period. If established, this discrepancy may probably be found to be due to a third body.—(*Lick Observatory Bulletin*, 105.)

Variations of Radial Velocities with Stellar Type.

During the course of the determinations of stellar velocities in the line of sight at the Mills Observatory in Chili, various changes in the appearances of the lines selected for measurement have been noted. Mr. S. Albrecht describes how from a discussion of numerous plates it has been concluded that many of these variations may be ascribed to the multiple character of the spectrum lines employed in the reduction. A very severe test for the validity of this view is the comparison of the types of stellar spectra, and it is shown with a considerable degree of certainty that there is a direct sequential variation of these special lines as we pass from stars of one spectral type to another. Now we know that many of the lines photographed in stellar spectra are multiple, although with the dispersion at present available the components may be indistinguishable. In passing from one type of star to another, one of these components may be strengthened or weakened relatively to the others, and thus alter the position of the centre of the blended line, which is measured by its maximum intensity. Doubtless in many cases such variations have been measured as velocity changes, whereas they may not be so. Examples are given of definite cases of this type of blended line in the region covered by the Mills photographs, λ 4258 to λ 4468, with the corresponding changes of intensity in Arcturus, the solar spectrum, and the spectra of sunspots, and also short notes on the appearance of the lines in other stellar spectra.

Variation of Jupiter's Rotation Period.

From a comparison of observations of the rotation of Jupiter during various periods, Mr. Stanley Williams finds a distinct difference. Thus the period determined from the series of observations for 1905-6 is 9h. 55m. 41.46s., while the 1904-5 observations resulted in the value 9h. 55m. 41.57s. The determinations from the Red Spot Hollow vary accord-

XUM

ing to whether they are made from one opposition to another or during each opposition. The difference may amount to 0.5 second. The observations were made with special care to avoid systematic error.—(*Astronomische Nachrichten*, 4,150.)

Visual Observations of Sunspot Spectra.

The Director of the Solar Physics Observatory at Kodaikanal has issued a further list of lines observed in the spectra of sunspots during the period 1905, July, to 1906, June. In the region under visual examination, λ4861-6573, 173 lines have been recorded, and in the table we have given particulars of their origin, number of spots in which each has been seen, number of times each line has been seen, and the mean amount by which the line is widened in the spot spectrum compared with its condition in the Fraunhofer spectrum.

An analysis of the lines showing the greatest degree of change indicates that the sunspot spectra during the last months of the maximum epoch were of substantially similar character to those seen during the preceding years throughout the last minimum.

Thus, the lines which are most affected are found to be due to Vanadium, Titanium, Chromium, and Scandium. There are a few cases where a dual origin is given for the lines, but in most of these one of the above elements is present, and greater spectroscopic power will be necessary to decide which of the components of the multiple line is concerned in the alteration of intensity.

Stars with Peculiar Spectra.

Professor Pickering gives a further list of 15 stars whose spectra are peculiar in some respect, chiefly with regard to the presence of bright hydrogen lines. One of these objects may possibly be observed with instruments of moderate aperture as its magnitude is given as 4.5. Its position for 1900 is R.A. 4h. 1.4m.; Decl. + 47° 28′. In this star the line H β (F) of hydrogen is bright.

Another star has been recognised as containing the special lines of the principal hydrogen series first observed in ζ Puppis. This is in Scorpio, but is only 7.1 magnitude. It shows bright lines at λλ4652 and 4698, and the dark principal hydrogen lines at λλ 4027, 4202, and 4544.—(*Harvard College Observatory Circular*, 124.)

BOTANICAL.

By G. MASSEE.

India-rubber.

A VERY interesting account of Heveas or Seringueiras, that is, rubber-producing plants of Brazil, is given by Dr. Rodriguez in the *Boletin del Minist. de Agric.*, Buenos Ayres. The employment of rubber dates from very early times. It was used by the natives for the fabrication of utensils and for games at the time when Columbus first visited America. Its use in America was introduced by Asiatics, who used rubber during prehistoric times. Each village carefully guarded its acknowledged share of the rubber-producing trees in the forest, valued on account of yielding milk which, when prepared, furnished material elastic and impermeable. For instance, the Mayas and the Nauhas of Mexico not only used rubber themselves, but also bartered this substance with other tribes in considerable quantity. According to the best historians, the towns situated on the Gulf of Mexico paid among other tributes to the Aztecs 16,000 loads of rubber each year. Among the different applications of rubber by the natives may be mentioned the manufacture of balls for the game called pela, played by various tribes in Southern Brazil. The earliest known name given to rubber was Ulé or Hule. The Indian tribe Karaybas, living in Guiana, called the rubber tree Heve, which was adopted by the French botanist Aublet as the botanical name of the genus to which the rubber tree belongs, *Hevea*. The Omanas, a tribe living in the Amazon region, manufactured for domestic uses from rubber tapers, balls, buckets, bottles, vessels for storing beverages, &c. It was through this tribe that the method of preparing rubber and its various uses were obtained by civilised people. Undoubtedly the first rubber known in Europe was brought from the East Indies, and up to 1820 its only use was that of removing pencil marks from paper, hence the name India-rubber. This rubber was obtained from a tree called *Ficus elastica*, Roxb.

A portion of the Omanas tribe of Indians migrated to Peru, where they encountered the tree known as *Castilloa elastica*, another rubber-producing plant. To this they gave the name of Cauacho. This word, in a corrupted form, has become the name by which the product of this tree is now universally known. Cauacho became converted into coatchu by the Spanish, and into cautchauc by the French.

The first use made of rubber by civilised people is attributed to missionaries living in damp parts of the Brazilian forest, who first covered their shoes with the milk of the rubber tree, and afterwards made their shoes entirely of rubber. In 1755 the use of rubber foot-gear was established in Para and in Lisbon.

Following the historical introduction, the author sketches the gradual development of the economic uses of rubber, geographical distribution of the various plants producing it, collecting and preparing for the market, &c.

Cocoanut Milk.

Dr. E. de Kruyff has described (*Bull. Dept. de l'Agric. Indes Néerl.*) his attempt to discover some use, on a commercial scale, for the watery substance contained in young cocoanuts. This attempt the author states was not crowned by success; nevertheless, some important results were arrived at during the investigation. The liquid is at first watery and clear, and with a sweet taste. In proportion as the nut ripens the liquid becomes denser, the sweet taste diminishes, and it assumes a frothy appearance. During this process a considerable amount of carbonic acid gas is disengaged. The liquid of the young fruit only contains amongst active sugars, a very small quantity of saccharose, and during the period of ripening this is converted into a glucose and levulose. This inversion of the saccharose is effected by the diastase sucrase, which is secreted by the cells of the albumen and becomes dissolved in the liquid. In addition to sucrase, oxydase and catalase are also present in the liquid, the two latter being alone present when the fruit is very young.

Scottish Peat Bogs.

An investigation of the plant remains in Scottish peat mosses by Mr. Francis J. Lewis shows that in the southern uplands the stratified plant remains show a gradual change from a woodland condition to that of heath and moss, and then back again to woodland. In some localities a bed of Arctic plants is interposed between the two woodland ages. A corresponding sequence of the beds in widely separated areas is considered as furnishing evidence of varying climatic differences during the decadence of the Glacial period. Arctic plants are everywhere absent from the base of the peat, which consists of a forest bed containing birch and hazel remains.

Coloration of Brown Algae.

A substance known as phycophaein has generally been considered as the material to which the colour of the brown seaweeds is due. Tswett, however, has recently stated (in *Ber. Deutsch. Bot. Gesellsch.*) that this substance does not exist in the living seaweed, and is not present in the chromatophore, but is a *post-mortem* product. A mixture of the following substances, present in the chromatophore of the living plant, gives the brown colour to the alga. Carotin, chlorophyllin, fucoxanthin, and fucoxanthophyl.

CHEMICAL.

By C. AINSWORTH MITCHELL, B.A. (Oxon.), F.I.C.

The Lighter Constituents of Air.

AN element termed "coronium" is said to have been found in the gases from the Solfatara of Pozzuoli, whilst the gases from Vesuvian fumaroles are stated to give spectra

containing lines not coinciding with any important line in the spectra of neon or helium. Hence an investigation has been made by Mr. J. E. Coates to determine whether systematic quantitative fractionation of large quantities of air would yield any constituent lighter than helium. A second object of the research was to redetermine by fractionation the amount of free hydrogen in air. In the experiments about 73,500 litres of air were concentrated in an air-liquefying apparatus, and the non-liquefying portion further fractionated by treating 70 to 80 litres at a time in glass bulbs immersed in liquid air boiling under reduced pressure, the volume being thus gradually reduced to 4,700 c.c. This residue was then sub-divided into further fractions by treatment with charcoal at temperatures of -110° C. to -190° C., and a final fractionation effected by means of liquid air at a temperature of about -205° C. The various fractions were examined spectroscopically, and it was found that there were no unknown lines in the spectrum of the lightest portion of air. As regards the amount of hydrogen, previous determinations by M. Gautier gave 19.5 volumes in 100,000, whilst Lord Rayleigh estimated the proportion at not more than three parts, and Sir James Dewar at not more than one part in 100,000. The amounts separated by Mr. Coates, however, were very much less, and he concludes that the proportion of free hydrogen in air is only about one volume in $1\frac{1}{2}$ millions.

The Distribution of Essential Oil in a Living Plant.

The odour of plants is due to active constituents in the volatile essential oils, the proportion of which varies at different periods of the growth. In order to study the formation and distribution of the essential oil MM. Charabot and Lalowe have made a series of determinations of the amounts present in different stages in different parts of the plant, taking absinthe (*Artemisia absynthium*) as typical of an odour-forming plant. They find that in the first stage, a long time before blossoming, the roots are free from essential oil, while the leaves contain about 11 times as much as the stalks. In the second stage, the beginning of blossoming, the roots become richer than the leaves in the oil, though all the organs show a considerable increase, the proportion in the leaves, for instance, being about doubled. In the third stage, advanced blossoming, the accumulation of oil in the roots is more pronounced, but there is a diminution in the stalks, leaves, and particularly in the blossoms, showing that there is a consumption of odoriferous constituents in the process of fertilisation. In a typical experiment the relative amounts yielded by the plant on July 10 and August 4 were 1,055 and 766 milligrammes. Hence it is evident that for the practical purpose of extracting the scent from flowers it is advisable to prevent fertilisation, or, better still, to extract the essential oil at an earlier period. In the fourth stage, when blossoming is over, the relative proportion of oil in the roots is greater, and there is also a slight increase in the proportion in the stalks. The small absolute increase in the amount of oil then yielded by the whole plant is to be attributed to the appearance of new leaves.

Bog Butter.

From time to time lumps of butter are dug out of the Irish bogs, and specimens of it may be seen in various museums. A chemical examination has recently been made by Messrs. Radcliffe and Maddocks of a sample of such butter, which was found four feet below the surface of a bog at Maghery, Co. Tyrone. The original lump, which weighed about 20 lbs., is probably some centuries old, and it is suggested that it had been put into peat water to preserve it, or to give it a flavour, and had then been forgotten. Yet so effectually had the fat been preserved by the peat water that it still retained many of the chemical characteristics of butter fat, though naturally its appearance had greatly changed. The exterior was white and granular, and the whole mass had been converted into a wax-like material. The conclusion that it had once been butter, however, was confirmed by the fact that numerous cows' hairs were present. A similar alteration of animal fats when buried in the earth for a long period has frequently been noticed, and the wax-like product is known as *adipocere*. The name originated with Fourcroy, who, in 1789, communicated to the French Academie des Sciences the results of his examination of the fat of some of the bodies transferred from the church graveyards to the Catacombs of Paris. The change in the fat was most noticeable in the case of bodies that had been buried about 15 years and originally crowded together, the bones being then coated with a gray plastic substance. Fourcroy came to the conclusion that this adipocere ("fat-wax") was formed by the slow decomposition of any animal substance, with the exception of bones, hair, and nails; but Chevreul, who studied the phenomenon in 1812, showed that only the fat originally present was affected in this way, and his conclusion was confirmed by Gay-Lussac.

GEOLOGICAL.

By EDWARD A. MARTIN, F.G.S.

Zeuglodon from the Barton Clay.

DR. C. W. ANDREWS has recently described to the Geological Society a cervical vertebra, found in the Barton Clay of Barton Cliff, which has been referred provisionally to *Zeuglodon Wanklyni*, a species described in 1876 by Prof. H. G. Seeley. The skull on which this description was founded is totally lost, so that this vertebra is the only bone of a *Zeuglodon* from the Barton Clay, and, with the possible exception of a vertebra from the Brockenhurst Beds (which is the type of *Balænoptera Juddi*), the only one found in the British Isles that now exists. In commenting on the discovery, Prof. Seeley remarked that no figure was given of his drawing of the parieto-frontal region in *Zeuglodon Wanklyni*, and he therefore drew attention to the circumstance that it was much broader and more convex on the upper aspect than in any Zeuglodonts subsequently made known from newer beds, and might possibly be referable to a distinct genus on this evidence. The caudal vertebra which he had described as *Balænoptera Juddi* he had long since chronicled as the caudal vertebra of a *Zeuglodon*, possibly identical with the Barton species. He referred also to the fact that in the Sedgwick Museum are similar caudal vertebræ of a Zeuglodont, which he believed were obtained by Mr. Henry Keeping from an excavation at Brockenhurst in the Brockenhurst Beds. Dr. Smith Woodward understood that the vertebra now found was obtained from a local collector, who had sold some associated vertebræ to casual visitors to Barton. He hoped that the publication of the paper would lead to the recovery of these specimens for scientific examination. Geologists will perhaps note.

Plateaux Around Torquay.

In speaking of the high level plains and plateaux near Torquay, Mr. Pengelly believed that there were several such plains at different levels, and thought that the time of their production was not very remote. On examination, however, his evidence breaks down, and the plateaux are regarded by Mr. A. J. Jukes-Browne as portions of one inclined plain.

Mr Browne describes the plateau-area as having an extent of about 3 miles in length from north to south, and about 2 miles in width. Its highest level is found at the foot of the ridge which forms the watershed between Torbay and the estuary of the Teign. In regard to the evidence as to the age of the planation, it is proved to be post-Permian, by the fact that Permian breccia forms part of the plateau-surface at St. Marychurch. It is also probably post-Cretaceous, because Cretaceous planation is not likely to have removed all the Permian. Its present dissected condition shows that it is older than the Pleistocene, and consequently an Eocene date would agree with local evidence.

Mr. Browne accepts the Eocene age of the Haldon gravels and of the Bovey deposits, and adduces evidence for a connection between the Haldon plateaux, and the Bovey basin on the one hand, and between the Aller basin and the Torquay plateau-area on the other. He regards the Bovey basin as a locally-deep depression, along a line of Miocene deflection which crosses Devon from north-west to south-east, and believes the plateau-area near Torquay to be the flattened-out extension of this flexure.

XUM

Channel Tunnel Geology.

After the lapse of 30 years, attention has again been drawn to the Channel deposits between Dover and Calais, arising out of a series of borings formerly made in connection with the proposed Channel tunnel. 7,600 samples were obtained by means of borings into the bed of the Channel, of which 3,267 were of value. It was found from these that the lines of outcrop of the various Cretaceous beds are practically parallel to a line drawn from Folkestone to Sangatte, whilst the dip is, as might be expected, towards the north, each formation in turn sinking beneath younger formations, as may be seen anywhere on the south side of the Surrey and Kent chalk escarpment.

The lowermost met with is (*a*) Gault and Upper Greensand (Fr. Albian), followed by the (*b*) Lower Chalk (Fr. Cenomanian), (*c*) the Middle Chalk (Fr. Turonian), and (*d*) the Upper Chalk (Fr. Senonian).

The strata are seen in the cliffs on the French coast between Escalles and Sangatte, and incline gently from the top of the cliffs to the beach in a north-easterly direction. The Gault and Upper Greensand are now regarded as equivalents of one another, possibly in some parts formed contemporaneously, under different conditions of sedimentation. The Gault is generally a dark, stiff, blue clay, and the Upper Greensand is a group of greenish sands and sandstones. At the base of the chalk is a well-defined band of Glauconitic or Chloritic Marl (Fr. Craie Glauconieuse) 11 ft. thick near Dover, 10 ft. 6 ins. thick at Sangatte, and this is a greenish chalk containing grains of Glauconite and Phosphatic Nodules. Above this lies a layer of Chalk Marl (Fr. Craie Marneuse), 23 ft. thick at Dover, 29 ft. at Sangatte, a clayey chalk, impervious to water, and then above this comes the great body of the Lower or Grey Chalk, called by the French Craie Grise and Craie de Rouen, at Dover, 87 ft. thick, at Sangatte 80 ft. It is a compact impervious stratum of greyish coloured chalk, containing no flints, and, so far as can be ascertained, free from fissures and slides. It is in this bed of chalk that it is proposed to construct the tunnel, as being a most excellent material in which to work, and one which possesses the peculiar property of gradually "puddling" itself and becoming impervious.

ORNITHOLOGICAL.

By W. P. PYCRAFT, A.L.S., F.Z.S., M.B.O.U., &c.

The Arrival of Summer Birds.

ALREADY bird lovers are straining eyes and ears to catch the first signs of the welcome arrival of our summer birds, and already one record has been made. This appears in the *Field*, March 16, wherein Mr. Percy Farrer relates how, on the night of March 3, he heard the call of the Stone-Curlew on Salisbury Plain. The writer is confident that he made no mistake in the matter, though he realises that this is a most remarkably early date. Though there is just a possibility that a mistake has been made, the record should be taken note of.

It would be well if observers would take more careful notes this year as to the weather and direction of the wind during, and preceding, the bursts of migration which will follow later.

Black Lark in Sussex.

At the last meeting of the British Ornithologists' Club three samples of the Black Lark (*Melanocorypha yeltoniensis*) were exhibited, these being the first recorded occurrences of this bird in Great Britain.

Two of these were exhibited by Mr. C. B. Ticehurst. One, an adult male, was killed on Pevensey Level on January 29, the other, a female, was shot near Lydd, Kent, January 31. The third example, a male, was obtained near Rye on February 16.

Though others were seen, these seem to have been the only specimens obtained.

Bittern in Wales.

Mr. C. H. Lloyd-Edwardes writes to the *Field*, March 2, from Nanhoron, South Carnarvonshire, to record the fact that a Bittern was "caught here last week." It was picked up in an exhausted condition, and carefully tended with a view to setting it free on its recovery. Unfortunately, however, it died next day.

Bittern near Eton.

In the *Field*, March 9, it is recorded that a Bittern "was recently shot on Dorney Common." Whether it has passed into the possession of the Eton Museum or not is not stated.

Eared Grebe in Belfast.

Mr. Ralph Leeke writes to the *Field*, March 9, to say that he shot an eared Grebe out of a party of three on Belfast Lough on February 20. A rare and accidental visitor to Ireland; this appears to be the sixth record of its occurrence on Belfast Lough.

PHYSICAL.

By ALFRED W. PORTER, B.Sc.

Standard of Radio-activity.

THE Röntgen Society has appointed a committee, with Prof. C. V. Boys, F.R.S., as chairman, to study the subject of radio-activity, with the view of obtaining, if possible, a standard of radio-activity suitable for those whose work includes the investigation or use of radio-active substances and X-rays.

The need for a means of testing the activity of Röntgen bulbs is felt especially keenly by those who use these in connection with therapeutics and surgery. A paper on the subject by Lord Blythswood and Mr. W. A. Scoble is printed in the February Journal of the Röntgen Society.

The Perception of the Direction of Sound.

Some thirty years ago, Lord Rayleigh made an extensive series of experiments in order to ascertain more precisely what are the capabilities of the ears in estimating the directions of sounds. It appeared that even with pure tones, the discrimination between right and left sounds could be made with certainty, and without moving the head. But with pure tones no pronouncment could be made if the sound were situated in front or behind the observer. With the speaking voice, front and back could often be distinguished.

The discrimination between right and left is usually supposed to be explicable by the greater intensity of sensation experienced by the ear which lies nearer to the sound. Lord Rayleigh considers that this explanation is adequate when the pitch is pretty high.

But for a frequency of 256, the difference in intensities at the two ears is far from conspicuous, and at 128 it is barely perceptible. Theory indicates that the difference of intensity in the case of a pitch of 128 should be decidedly less than one per cent. of the whole; yet the discrimination between right and left is as easy as before.

Lord Rayleigh has recently returned to the question, commencing with a frequency of 128. Every precaution was taken to remove bias. The observer, either on his feet or more conveniently seated upon a rotating stool, is turned round until he loses his bearings. It is often necessary to cover the eyes with the hands as well as to close the eyelids, in order sufficiently to exclude the light. With different observers it was a moot point whether there was any balance in intensity for the source on the same side as the open ear in the case of frequency 128, while with 256, the difference was marked.

Thus there are cases where it is impossible to attribute the correct judgment of direction to the relative intensity. The only other circumstance in the case of pure tones is the relative phase of the notes at the two ears. The sound reaches one ear later than the other, and the same statement holds good for all the successive condensations in the wave train. If this phase difference is admitted as a source of the

power of discrimination, it must be limited to sounds of low pitch. The retardation for frequency 512 would be about half a period, for 1024 a whole period, and so on; and since any number of whole periods are equivalent to no retardation at all, such a retardation can be of no avail as an explanation. In testing the applicability of the explanation to low pitches, two forks, electrically maintained, were placed in separate rooms, and the sounds led through composition tubing to the observer's ears. The forks were slightly out of tune, *so there was a progressive increase of phase difference* between the notes. The time for the retardation to change through a whole period was about five seconds. On the very first trial, Lady Rayleigh and Lord Rayleigh at once experienced a distinct right and left effect, *the sound appearing to transfer itself alternately from the one side to the other.* The beat may be slowed down until it occupies 40 or even 70 seconds, with the same results. Further experiments showed that the transitions between right and left effects correspond to agreement and opposition of phase. When the vibration on the right is the quicker, the sensation of right follows agreement of phase and that of left follows opposition; and right and left may be interchanged in the above statement. These results, it may be repeated, apply only to pitches of 128 or under. At a pitch of 256 the effect is still present, but begins to be complicated by the differential intensity effect.

These and a large number of other experiments are described by Lord Rayleigh in the *Philosophical Magazine* for February.

ZOOLOGICAL.

By R. LYDEKKER.

Domesticated Cats.

ACCORDING to a paper recently read before the Zoological Society by Mr. R. I. Pocock, two distinct types of so-called tabby cats are recognisable. In the one the pattern consists of narrow vertical stripes; and in the other of longitudinal or obliquely longitudinal stripes which, on the sides of the body, tend to assume a spiral or sub-circular arrangement characteristic of the blotched tabby. One or other of these types is to be found in cats of almost all breeds, whether Persian, short-haired, or Manx; and there appear to be no intermediate stages between them. Cats of the striped type are no doubt descended from the European and North African wild cats; but the origin of cats exhibiting the blotched pattern appears to be unknown. As it was to a cat of the latter kind that Linnæus gave the name *Felis catus*, the author urges that this title is not available for the European wild cat, which he would call *Felis sylvestris*.

The British Hares.

In our last issue reference was made to the recognition of the racial distinctness of the British badger. A similar rank has been accorded, by Mr. Max Hilzheimer in the *Zoologischer Anzeiger*, to the Scotch hare, for which the name *Lepus medius scoticus* has been suggested, although *L. timidus scoticus* appears preferable. It may be added that the Irish blue hare forms a race by itself, *L. t. hibernicus*, characterised by not turning white in winter; while the common hare of the south of England has been separated from the Continental form as *L. europæus occidentalis*.

Parental Instincts in Fishes.

Several years ago an article by the present writer on the nursing habits of fishes and frogs was published in "KNOWLEDGE." As regards freshwater fishes, the subject has been treated in much greater detail in a most interesting paper by Mr. Theodore Gill, recently published in the *Smithsonian Report* (Washington) for 1905. Here nests and nurseries of all kinds—from the simple piece of ground cleared on the river or lake bottom by the American bowfin or the Grecian glanis, to the bubble-nest of the Malay fighting-fish, or the still more elaborate structure formed by our own stickleback—will be found described in a manner which can scarcely fail to attract the attention and arouse the interest of every reader. The importance of papers of this nature, at a time when the attention of collectors and travellers is too much concentrated on the acquisition of specimens, to the neglect of the habits of the animals to which they belong, can scarcely be over-estimated. It is to be hoped that the author will favour us with another communication dealing with the nursing habits of sea fishes.

The Teeth of the Platypus.

Science, it is sometimes said, advances so rapidly nowadays that it is almost impossible to keep pace with progress. There are, however, cases where its rate of progress is tortoise-like rather than hare-like; not, we hasten to add, from any slackness on the part of its devotees, but from difficulties connected with the acquisition of specimens and the time necessary for the investigations. A remarkable instance of this slowness is afforded by the investigations connected with the existence of the teeth in the Australian platypus, or duckbill. The existence of such teeth was first announced by Prof. E. B. Poulton, of Oxford, so long ago as the year 1888, and yet it has taken from that time to this to ascertain how many of such teeth (or, rather, germs of some of them) the creature really possessed, and even now there are some points requiring further elucidation. Hitherto the number of teeth has been supposed to consist of either three or four pairs in each jaw, but Messrs. Wilson and Hill, in a paper recently contributed to the *Quarterly Journal of Microscopical Science*, have demonstrated that the set functionally developed originally consisted of five pairs. Of far more importance is their discovery that this functional series was preceded by a simpler series of milk-teeth. It, therefore, follows that the platypus, and probably also its sole living relatives the spiny anteaters, or echidnas, are descended from mammals (or, shall we say, half-mammals and half-reptiles?) with a complete double series of teeth; the toothless condition of the adults of both being a purely adaptive character.

Papers Read.

The whole time of the meeting of the Linnean Society held on February 21 was taken up by Mr. J. S. Gardiner's account of the result of his expedition to the Indian Ocean in 1905. Special attention was directed in this communication to the land-fauna of the Seychelles.

Interest at the meeting of the Zoological Society on February 5 was largely concentrated on what Prof. Ray Lankester had to say concerning giraffes and okapis. It was shown that the paired horns of the giraffe are situated on the parietal, and not on the frontal, bones; while it was further demonstrated that the small, bare, polished tips of the male okapi's horns are strictly comparable to the antlers of deer, although this must not be taken to imply that the two structures necessarily have a common origin. In reference to the recent discovery that the hair of the face is striped in fœtal giraffes, it was mentioned that a kind of longitudinal striping or shading is noticeable on that of some adult individuals. Small mammals from the Philippines formed the subject of a paper by Mr. Thomas; while Mr. Boulenger directed attention to certain peculiarities in connection with a tree-frog from Amazonia, which it was proposed to call *Hyla resinifictrix*. The specific name refers to the fact that the species is in the habit of constructing resinous cups in the hollow branches of forest trees. In these cups rain-water collects, thereby affording a suitable home for the eggs and tadpoles. The resin is yielded by the bark of several kinds of trees. A number of new crustaceans, chiefly from New Zealand and the Gulf of Siam, were named by Dr. Calman.

At the meeting of the same body on February 19 the colour-markings of domesticated cats served as a text for the highly interesting discourse by Mr. Pocock, to which a paragraph is devoted above.

An abnormal turbot formed the subject of a paper by Mr. Cunningham; the unpaired (azygos) veins of mammals were discussed by Mr. Beddard; while Baron Francis Nopsca contributed notes on flight in animals. He urged that from the mechanical point of view a patagium, or flying-membrane, and a set of flight-feathers are different organs; and also pointed out the osteological analogies between bats and pterosaurs on the one hand, and between birds and dinosaurs on the other. It was suggested that bats and pterosaurs had arisen from leaping, arboreal forms, whilst birds had come from a terrestrial, running stock.

XUM

REVIEWS OF BOOKS.

ANTHROPOLOGY.

Natives of Australia, by N. W. Thomas; pp. xii. and 256, illustrated (London: Constable and Co., Ltd., 1906; price 6s. net).—The issue of a series of volumes (of which the one before us is the first) devoted to "The Native Races of the British Empire," and written, whenever possible, by contributors personally acquainted with the particular branches of mankind of which they treat, is an excellent idea, and one, if well carried out, worthy of the most generous support on the part of the reading public. Indeed, it may serve in some degree to mitigate the sting of the reproach uttered by the late Sir William Flower that we, as a nation, have fallen short of our opportunities and our duty in the matter of studying the aboriginal nationalities of "Greater Britain." In the present volume the author, who has previously written on Australian native customs, commences his task with a brief account of the physiography, fauna, and flora of the country, followed by observations on the physical characteristics and relationships of the aborigines. In the latter connection it is unfortunate that, while referring to Sir William Flower's opinion as to their Melanesian affinities, Mr. Thomas has apparently overlooked the theory independently advocated by Mr. Lydekker (in this journal) and Dr. A. R. Wallace, and subsequently endorsed and amplified by Dr. Gregory (in the *Dead Heart of Australia*), as to the Caucasian descent of the "black fellow." With the exception of this unfortunate oversight, the book appears in the main to be well up-to-date, and affords a fund of seemingly trustworthy information with regard to the manners and customs of these interesting people. The author has, of course, much to say with regard to the Arunta of South-Central Australia, a tribe wholly unknown to the earlier explorers, and whose very existence has only been revealed of late years. The illustrations, both of individual aborigines and of native customs and implements, are for the most part excellent; but it would have been an improvement if, in the case of portraits of individuals, both full-face and profile views had been given. In this volume an important undertaking makes a good start, and if its successors only maintain the same level, the success of the undertaking ought to be assured.

CHEMISTRY.

A Method of Teaching Chemistry in Schools, by A. M. Hughes, B.Sc., and R. Stern, B.Sc.; pp. xii. and 120 (Cambridge: University Press; 3s. net).—The method outlined in this most suggestive little book is to proceed from the known to the unknown, allowing the children, as far as possible, to make their own experiments and to draw their own deductions from the results. For instance, a piece of brick suggests the demonstration of the properties of clay; bricks are baked on a small scale from clay, and the children are led to devise experiments to prove that water is driven off in the process. This leads on to distillation, solution of salts, and crystallisation. The heating of iron sulphate crystals to expel the water of crystallisation produces an acid liquid, and this serves as an introduction to the nature of acids, from which it is a step to alkalies, neutralisation, and the nature and preparation of salts, and so on. This is unquestionably the right way to make children realise that chemistry is not merely a science to be learned as a school task, but something connected with the familiar things of everyday life, and, consequently, it will fix their interest. At the same time the authors are justified in their belief that by following their experimental method the foundations will be laid for a thorough understanding of the principles of theoretical chemistry later on. While we have nothing but praise for the method as a whole, a few criticisms may be made upon the details. The outlined lessons strike us as being insufficiently graduated, or, rather, as applicable to children of very different ages. For instance, it is proposed at a very early stage that the class should be set to determine the solubility of salt and nitre in water at different temperatures and to draw solubility curves. In our opinion anything in the way of calculations and curves at this period would repel a young child. A few experiments to illustrate the indestructibility of matter and the different states of matter seem far more appropriate at this point, and these experiments should be devised so as to make the required facts manifest to the eye, without the necessity of calculations. A minor objection is that the child is to be taught from the first to distinguish between organic and inorganic substances. Would it not be better to accustom it to speak of *carbon* and *non-carbon* compounds, seeing that the old barrier between organic and inorganic chemistry has disappeared? But apart from these points, which are matters of opinion, we regard this book as a most valuable contribution as to how chemistry should be taught, and it is to be remembered that the authors evidently intend it as a suggested outline which each teacher can modify, rather than as a prescribed course to be followed blindly.

HISTORY.

Monumenta Orcadica: the Norsemen in the Orkneys and the Monuments They Have Left, by L. Dietrichson. With 152 illustrations (London: Williams and Norgate, Henrietta Street, Covent Garden, W.C.; £3 net; quarto).—The Norwegian text of this sumptuous volume, consisting of the last 206 pages, is prefaced by an abridgment of the same in English, and contains 77 pages. The whole constitutes a monumental work, and to the student of the early connection between the Norsemen and the northern extremities of our islands this work is of intense interest. The Earls of Orkney, so long as they were subject to Norway, were always, at the command of the ruler, able to harry the mainland, or the Hebrides, or Ireland, and many well-known places in Scotland have been celebrated by reference to them in Norwegian sagas. Book I. describes the remains, pre-historic and Celtic-Christian, which the Norwegians found on their arrival at the Orkneys. Book II. contains descriptions of Norwegian monuments reared under Celtic influence (872 to 1200 *circ.*) and those raised under Anglo-Norman and Gothic influence (1137-1468). In this period is included the building of what is called the "grandest building in the Orkneys," viz., St. Magnus' Cathedral at Kirkwall. In Book III. there is a description of "Monuments from the Scottish Period," that is, after 1468. The illustrations, many of which are photographic reproductions of excellent drawings of the architecture of the various buildings dealt with, give additional interest to the letterpress. The description and illustrations of the pre-Norwegian sepulchral chambers (Pict-houses) are particularly interesting to the student of primitive man. These chambers were excavated in close resemblance to the dwellings of the living, and are of the same shape as others of Neolithic age found over Northern and Western Europe. The articles found in the graves are also of Neolithic age, and there are supposed to be no less than 2,000 of such graves. The book forms a massive quarto volume, and we hope that it will secure a demand from the public libraries of this country.

METEOROLOGY.

Peruvian Meteorology, 1892-1895. Compiled and prepared for publication by Solon I. Bailey (Cambridge, U.S.A., 1906; 4to. 140 pp. and 5 plates).—This forms Part II. of Vol. XXXIX. of the *Annals of the Astronomical Observatory of Harvard College*. Few systematic meteorological observations were made in Peru during the latter part of 1890 and throughout 1891. Observations were resumed at Mollendo (80 feet) and Arequipa (8,050 feet) early in 1892, and at about the same time a station was placed at La Tova (4,140 feet), midway between the above stations, on the Desert of Islay. In January, 1892, a station was established on the slope of Chachani, at an elevation of 16,650 feet, and in October, 1893, another on the summit of the Misti, at 19,200 feet, with auxiliary stations at 15,600 feet and 13,300 feet. This system of stations was extended in 1894 by establishing stations at Cuzco (11,100 feet), between the two great ranges of the Andes, and at Santa Ana (3,400 feet), to the east of the Andes, in the valley of the Urubamba, a tributary of the Amazon. All the stations were provided with Richard Frères self-recording barographs and thermographs, and Green standard, maximum and minimum thermometers. Records were also made of the cloudiness, the direction and estimated force of the wind, the rainfall, and of such special phenomena as fogs, halos, storms, and earthquakes. The present volume contains only the eye observations at these stations. The records of the self-recording instruments will be published in a later volume. During the three years and nine months of the observa-

tions at La Joya no rainfall greater than a trace (*i.e.*, less than half a hundredth of an inch), was recorded.

NATURAL HISTORY AND SPORT.

Hunting and Shooting in Ceylon, by Harry Storey (and others). (London: Longmans, Green, and Co., 1907; pp. xviii. and 366, illustrated; price 15s.).—Although possessing no big game animals specifically distinct from those inhabiting the Indian Peninsula, while it lacks several of the finest and handsomest of the latter, such as the tiger, the rhinoceros, and the black-buck, Ceylon has many attractions of its own as a hunting field, not the least of these being the beauty of the scenery amid which the sportsman carries on his pursuit. The four-footed game animals range in size from the lordly elephant (albeit in most cases belonging to the practically tuskless variety) to the muntjac and the still more diminutive memimna, or mouse-deer, and likewise include that most beautiful and characteristic of all Indian ruminants, the spotted deer, or chital, as well as the noble sambar deer. Quail, snipe, and ducks of many kinds abound during the winter season, while handsome fruit-pigeons of several species, as well as smaller doves, are to be had at all times of the year. Years ago, when all these various kinds of game were to be met with in plenty, the island must, indeed, have been little short of a sportman's paradise. To-day, although the migratory game-birds doubtless arrive in numbers but little, if at all, diminished, the big game has suffered woefully from the continued assaults of Europeans and natives; the latter, according to the author, being those who have worked most harm in the case of medium-sized species like the spotted deer. If the game is not to be practically exterminated, prompt and vigorous action on the part of the Government is, writes Mr. Storey, a matter of imperative necessity; we sincerely hope the warning will not be disregarded. Although Mr. Storey, like the other sportsmen whose assistance he has invited, for special sections, does not profess to be a naturalist, yet he incidentally gives many pieces of information which should be of great value to the working zoologist; while, as a sporting account of the game animals and birds of the island, his volume can scarcely be surpassed. From title-page to index it is written in a fresh and agreeable style, and may be said to be almost redolent of the jungle, so graphic are many of the descriptions of hair-breadth escapes and startling incidents. Of course, there are a few misprints in the case of scientific names, such as *Rhyncœa* for *Rhynchæa*, but these detract but little from a delightful work.

SCHOLASTIC.

Clive's Mathematical Tables, pp. 49; price 1s. 6d.; and **A Text-Book of Light,** by R. Wallace Stewart, D.Sc.—fifth impression revised and enlarged by John Satterly, B.Sc.; pp. 346; price 4s. 6d. (London: The University Tutorial Press, Ltd., 1906).—As recently as about a year ago we had great difficulty in obtaining good and cheap small mathematical tables for class use. One set contained natural sines, cosines, and tangents, but not their logarithms; another contained sines but no cosines, and so on; in every case the result was to necessitate the use of methods which an experienced teacher would condemn. There has recently been an epidemic of tables reproducing on a small scale the previous epidemics, first of euclids, and then of school geometries, and some of the tables have actually met the requirements. The book brought out under the editorship of Dr. Briggs (a namesake, if no relative, of the introducer of the decimal system of logarithms), has several advantages. It is the first book we have seen in which sines and cosines are to be taken from the same table using the right-hand column and bottom line for the angles, whose cosines are required or are given. For teaching purposes this plan is very desirable. It has the further advantage that cotangents, secants, and cosecants are included without requiring more tables than are commonly used for sines, cosines, and tangents. This enables the student to add the logarithm of a cosecant instead of subtracting that of the sine, and thus to solve a triangle by a single addition sum instead of separate additions and subtractions. We should like to see a table of complementary logarithms added for a similar purpose. The features specially claimed in the preface are (1) that the results are given to five decimals instead of four as is usual in such tables; (2) that corrections to the tables of mean differences are inserted where these differences change rapidly; and (3) that the mean differences are omitted where they become too irregular for this method. The rest of the book is taken up with explanations of the use of tables. Dr. Wallace Stewart's Text-Book of Light has been so popular and so universally appreciated that it needs little recommendation at the present time. Mr. Satterly's new edition quite maintains the excellence of the old book, and is, in addition, carefully brought up to date. On reading the book we cannot help regretting that geometrical optics instead of being included in courses on elementary mathematics is called "Light," and taught as a branch of physics. The construction of reflected and refracted rays is a valuable exercise in constructive geometry, the calculation of their directions affords practice in the use of tables, and the subject, though essentially mathematical, is capable of easy experimental verification. The reason for the present state of affairs is probably that most mathematicians cannot do optics. This is because it has been usual to introduce into the subject complicated mathematical formulæ, involving, among other things, determinants, instead of treating it on modern lines with the aid of exercises of a constructive character.

First Stage Human Physiology, by G. Norman Meachen, M.D., B.S. (W. B. Clive, 157, Drury Lane, W.C.; 2s.).—This small volume, one of the Organised Science Series, will meet the requirements for which the author intended it. It is designed with the object of combining the advantages of theoretical knowledge with practical instruction, and is intended primarily for candidates desiring to enter for the examination in physiology (Stage I.), of the Board of Education. The various chapters have been carefully perused, and there is little to find fault with; the subject matter is well expressed, and is correct in detail; the text also is good, and it is anticipated that a more extended circle of readers will be found than the Board of Education will require. At the end of each chapter is to be seen a most useful series of questions, which will enable the student to gauge his capacity for absorbing knowledge as he advances. The diagrams are clear and intelligently drawn, with the exceptions, perhaps, of the spleen and diaphragm; but this is a small matter, and in no way detracts from the general excellence of this little work. To those teaching, or desiring to learn, the elements of Human Physiology, it can be confidently recommended.

MISCELLANEOUS.

The Royal Society, by Sir William Huggins, K.C.B., O.M., &c. (Methuen; 4s. 6d. net).—This work, by the recently-retired President of the Society, consists mostly of selections from four of the Presidential Addresses delivered by the author. These are intended to "rouse attention to one of the most important of the practical questions of the day, if we are to fulfil our mission as a great nation—the necessity of giving science its proper place in all education." We need hardly add our entire endorsement of the importance of this matter, and can only hope that this pleasantly written, yet authoritative account, will be widely read by those able to give furtherance to the cause of education. The first part of the book is appropriately occupied with a short history of this great institution, which was originally formed in 1645 as a small club. It is noteworthy that one of the original titles was "The Royal Society of London for improving of Natural Knowledge by Experiment." A number of good illustrations add to the value of the work, including many photographs of interest and some pretty initial designs by Lady Huggins.

We have to acknowledge the receipt of five fasciculi of the publications of the University of California, two of these being "Technical Bulletins" on Entomology, while the other three are "Contributions from the Laboratory of the Marine Biological Association of San Diego." Since all the papers are of a purely technical nature, chiefly of interest to specialists, it must suffice to refer to their subjects. Of the two entomological papers, one deals with the nature of the veins in the wings of insects, and the second with the flies of the family *Ephydridæ*. Two of the fasciculi from the marine laboratory are devoted to the consideration of various crustaceans; while the third treats of certain "giant animalculæ" constituting the group Dinoflagellata.

XUM

MICROSCOPY

Conducted by F. SHILLINGTON SCALES, B.A., F.R.M.S.

Royal Microscopical Society.

FEBRUARY 20th.—The Rt. Hon. Lord Avebury, P.C., F.R.S., President, in the chair. Mr. J. W. Gordon read a paper entitled "An early criticism of the Abbe Theory," which was written as an answer to a paper with a similar title read before the Society by Mr. Conrady on October 17, 1906. At the close of the paper Mr. Gordon exhibited on the screen some photographs of the spectrum produced by the fine ruling of an Abbe diffraction plate. In the following discussion Mr. Conrady and Mr. Rheinberg took part, and Mr. Gordon replied. Dr. Hebb read a paper by Mr. Jas. Murray on "Some Tardigrada from the Sikkim Himalaya," and another by Dr. Eugene Penard on "Some Rhizopods from the Sikkim Himalaya," mounted specimens being shown under microscopes of species found in moss at a height of about 3,000 feet above sea-level. Descriptions of mites, by Mr. N. D. F. Pearce, and of Rotifers, by Mr. Jas. Murray, found in the same material, have already been read before the Society. Dr. Hebb read a letter from Mr. E. M. Nelson, which was an appendix to his paper on the flagella of the tubercle bacillus. Dr. Hebb also read an extract from a letter from Major Sampson, now in Southern Nigeria, describing an incident in ant life that came under his notice. He saw "a thick, living arch of travelling ants across a sunny road, and in the centre hundreds of pupæ being carried along in the shade caused by this arch. This is the more notable because the African ant cannot generally stand the sun at all." Some microscopes belonging to the late Prof. Lionel S. Beale were exhibited, and there was also an exhibition of slides, mainly of marine life, lent by Mr. Flatters.

Quekett Microscopical Club.

February 15.—The President, Dr. E. J. Spitta, F.R.M.S., &c., gave, as his annual address, "A Review of Photomicrography." Mention was made of early attempts in this direction, such as Daguerreotypes of blood corpuscles, &c., by Leon Foucault, taken by the electric light in 1844, and others by Donné, of Paris, obtained with the solar microscope about the same time. Early difficulties, such as the difference between actinic and visual foci, and the large amount of outstanding aberration of all kinds in even the best objectives then available were referred to. The great advances made in the "art" consequent on the introduction by Abbe of the apochromat and semi-apochromat, and, more recently, by the improvements in the manufacture of photographic plates and contrast screens were then dealt with. In connection with these latter subjects the President especially mentioned the names of Mr. Sanger-Shepherd, Dr. Mees, and Mr. Thorne-Baker. The convenient division of the subject into three divisions of low, medium, and high-power work was pointed out, and in conclusion the President exhibited a very fine series of his own photomicrographs ranging from low-power work to the very highest, including a series of eight photographs of *Amphipleura pellucida* taken with various screens and different lighting arrangements, the last four showing the diatom photographed in dots with a magnification of about 3,000 diameters.

The Use of the Bull's-eye Condenser.

The bull's-eye condenser is invariably found in any microscopic outfit, but it is surprising how little is known of its uses. As a matter of fact, as ordinarily met with, it is optically very imperfect, its spherical and chromatic aberrations being very considerable, and it is, therefore, worse than useless for critical work. Workers with high-power lenses often complain of insufficient light, and, instead of carefully adjusting the sub-stage condenser, try to increase the quantity of light by methods which are more or less ineffective or even stultifying. I have frequently come across such workers using an incandescent gas burner, or an incandescent electric lamp, and interposing between such an illuminant and the microscope an ordinary uncorrected bull's-eye without any regard for its focal length and consequent adjustments. It may be of service if I deal, therefore, with a few points concerning the use of the bull's-eye, and incidentally also with the sub-stage condenser.

The sub-stage condenser of the microscope is adjusted for slightly divergent light, and the very best aplanatic condensers are corrected for a source of illumination about eight inches away, and also for a definite thickness of slide. If, as is usual, a plane mirror is interposed, this must be taken into account, and the distance from the illuminant to the mirror and thence from the mirror to the condenser will make up the total eight inches. Any considerable divergence from these conditions will reduce the aplanatic cone that the sub-stage condenser is capable of transmitting, and such a condenser, with a total N.A. of 1.0, and an aplanatic cone of, say, .96, will now only pass an aplanatic cone of .9 or even less. Of course, this is only important on critical work, and the uncorrected chromatic condenser of Abbe, with its aplanatic cone of less than .5, is but little sensitive to such matters. But the aplanatic condenser above mentioned can, to a certain extent, be re-corrected for such divergences by slightly unscrewing the top lens and thus re-adjusting the combination. When used with parallel light the focus of such a condenser is, of course, slightly less, but it is always well to bear in mind that its corrections are somewhat less perfect unless it be used as intended by the makers.

It should be unnecessary to explain that a sub-stage condenser must be invariably used with the plane mirror, but it is astonishing how many people believe that the use of the concave mirror is necessary with artificial light, and how many otherwise useful handbooks of histological and bacteriological methods perpetuate the error. I have not only seen but heard it stated that with daylight the plane mirror should be used, but that with artificial light the concave mirror is necessary. As a matter of fact, with ordinary daylight it makes little difference whether the plane or concave mirror be used, either with or without the sub-stage condenser, as the light is too diffuse to be focussed accurately. With artificial light, however, the concave mirror should be used when the sub-stage condenser is removed, as by that means the source of illumination is focussed (though not accurately) upon the object, providing that the mirror be adjusted upon the tail-rod to do this. The simplest means is to put a piece of thin note-paper on the stage and to adjust the disc of light until it is at its smallest diameter.

But when the sub-stage condenser is used with artificial light the concave mirror would throw upon it strongly convergent light instead of slightly divergent light, and the loss of light, to say nothing of the interference with the corrections of the condenser, would be very great. Of course, parallel light could be obtained by bringing the lamp so close to the microscope as to be in the principal focus of the concave mirror, but the inconveniences of such a method are manifest, whilst the corrections of the condenser are again disturbed, and even more seriously, by the resulting astigmatism.

Now, what will happen if we interpose between the lamp and the microscope an ordinary bull's-eye? The bull's-eye is plane on one side and spherical on the other, and if used at hap-hazard, as is generally the case, conjugate foci are set up, and the mirror (of course, the plane mirror) receives again convergent light, the degree of convergence varying according to the position of the bull's-eye. To endeavour to adjust so ill-corrected a condenser to give slightly divergent light is impracticable, so our only alternative is to try and give approximately parallel light. To do this the bull's-eye must be brought quite close to the lamp, so that the illuminant is in its principal focus, and it is surprising how short this focus is, and how near the bull's-eye is to the lamp in consequence. Moreover, the position of least spherical aberration in such an uncorrected lens is when the *plane* side is turned to the lamp, so this must be attended to, and, of course, the lens must be set square with the axis of the light.

To find this position the room can be darkened and a piece of white paper brought into the beam of light. If the bull's-eye were really properly corrected one could obtain by trial a position where the illuminated disc projected on the paper did not vary in size when the latter was moved backwards or forwards, but owing to its aberrations this will be found impossible, and we must take, therefore, the position of least divergence. To look directly at the bull's-eye and expect it to be filled with light is useless, because the eye can utilize the approximately parallel rays and accommodate itself to them.

Of course, there are bull's-eye condensers which are much better corrected than the ordinary kind, such as the Nelson and Herschell type, but even these are imperfect, and the only two known to me which are really strictly corrected and will give truly parallel light, or even project an aërial image of the source of illumination, are Mr. Conrady's condenser, made by W. Watson and Sons, and one since brought out by R. and J. Beck.

The bull's-eye has another use, however, and that is to illuminate opaque objects, and for this purpose the plane side should be turned towards the object and brought quite close to it. Perhaps the best means of illuminating such opaque objects, however, is to combine bull's-eye and parabolic mirror; a method only slightly inferior to the older Lieberkühn. For this purpose the lamp is put at a level rather above the stage of the microscope, and the bull's-eye adjusted to give approximately parallel light; this light is directed almost horizontally directly upon the parabolic mirror, the latter, in turn, reflecting it downwards upon the object. It should be remembered that the focus of the parabolic mirror is very short also, so it must be quite close to the object. Those who have circular tail-rods on their microscopes will find it an excellent substitute for the parabolic mirror if they twist their concave mirror above the stage and use it instead.

Digestion in Drosera.

R. Dubois has put forward the theory that the digestive powers of *Nepenthes* are probably due to enzymes secreted by bacteria, and recent researches by a French worker, E. Labbe, show that something of the same nature takes place in *Drosera*, though the bacteria play only a secondary part. Moulds, such as *Aspergillus glaucus*, *Penicillium glaucum*, *Mucor mucedo*, and *Cladosporium herbarum*, with a few bacteria, were found to be present, and to excite fermentation of the sugar secreted by the glands of the plants, and to give origin to certain acids. A proteolytic ferment which peptonised fibrin was found in the secretion.

Notes and Queries.

T. H. B., Lewisham.—There is no reason why a simple lens of 2 cm. focal length should not serve you, to a certain extent, as a condenser, but, of course, its angular aperture would, as you suggest, be too small to fill any but a lens of equally low aperture. You will have no difficulty in measuring its aperture by direct measurement, or by testing it with various objectives, bearing in mind that its aplanatic aperture is of importance also. If you have any difficulty in doing this I shall be glad to help you, but I do not think I need examine the condenser for you, as your description is quite sufficient. I am afraid you will find it greatly inferior in performance to even an ordinary Abbe condenser, and in comparison you would note at once the much smaller quantity of light it passes.

T. R., Durham.—I know of no cement or varnish which will withstand glycerine or glycerine jelly mounts for more than a variable time, and I have tried all kinds. Perhaps marine glue is the best, but it is annoying stuff to use. Sooner or later the cements give way, and I have periodically to go through my cabinets and remount some of my slides or to re-ring those which are beginning to look shaky. With regard to air-bubbles, you will have to bear in mind that glycerine jelly, unlike Canada balsam, does not absorb such bubbles, and that even the very smallest must therefore be got rid of before completing the mounting. There are several ways of doing this. First, the soaking of the section for a considerable time in freshly-boiled water. The boiling of the water expels the air in solution, and therefore the air in the specimen is taken up instead. Secondly, the soaking of the section in a mixture of glycerine and water, before the final mounting. Thirdly, the careful examination of the specimen before the cover-glass is put on. There is yet another method, which I nearly always utilize myself, but which requires both care, practice, and judgment. Place a slide upon a hot brass table (the usual brass mounting-table with a lamp beneath), transfer the section to its centre, add a small piece of glycerine jelly, heat the table gently until the jelly melts, and put on the cover-slip. Then with the point of a fine dissecting needle press gently on the cover-glass to keep it and the section beneath it in place, and raise the temperature of the hot table until tiny bubbles begin to appear in the film of liquid glycerine jelly and to make their way towards the periphery of the cover-glass. Remove the lamp at once and shift the slide to a cooler part of the plate. It will be found that these bubbles have generally carried away with them any troublesome air-bubbles, but, if necessary, the process can be repeated. It will be manifest that this procedure requires care, but in my own practice I do not find it harmful to even delicate structures, whilst it has enabled me to make perfect mounts of otherwise hopeless objects. With regard to the obliteration of detail by using clove oil, I think the Canada balsam is probably more responsible. It is a question of contrast in refractive indices. Try some other mounting medium, or content yourself with less perfect clearing, by such a reagent as turpentine. As long as the specimen is free from water you can mount in Canada balsam direct.

[*Communications and inquiries on Microscopical matters should be addressed to F. Shillington Scales, "Jersey," St. Barnabas Road, Cambridge. Correspondents are requested not to send specimens to be named.*]

XUM

The Face of the Sky for April.

By W. SHACKLETON, F.R.A.S.

THE SUN.—On the 1st the Sun rises at 5.40 and sets at 6.30; on the 30th he rises at 4.38 and sets at 7.18. The equation of time is negligible on 16th, hence this is a convenient date for the adjustment of sundials.

Sunspots and faculæ have of late been very conspicuous on the solar disc, whilst recent spectroscopic observations of the limb have shown many prominences.

The position of the Sun's axis, equator, and heliographic longitude of the centre of the disc is shown in the following table:—

Date.	Axis inclined from N. point.	Centre of disc S. of Sun's Equator.	Heliographic Longitude of Centre of Disc.
April 1 ..	26° 19′W	6° 31′	32° 22′
,, 6 ..	26° 27′W	6° 13′	326° 23′
,, 11 ..	26° 24′W	5° 52′	260° 24′
,, 16 ..	26° 10′W	5° 29′	194° 23′
,, 21 ..	25° 46′W	5° 3′	128° 21′
,, 26 ..	25° 10′W	4° 35′	62° 17′

THE MOON:—

Date.	Phases.	H. M.
April 5 ..	☾ Last Quarter	3 21 p.m.
,, 12 ..	● New Moon	7 6 p.m.
,, 20 ..	☽ First Quarter	8 38 p.m.
,, 28 ..	○ Full Moon	6 5 a.m.
,, 3 ..	Perigee	4 36 a.m.
,, 18 ..	Apogee	5 18 p.m.
,, 30 ..	Perigee	1 30 p.m.

OCCULTATIONS.—The following are the occultations of the brighter stars visible at Greenwich before midnight.

Date	Star's Name.	Magnitude.	Disappearance.		Reappearance.		Moon's Age.
			Mean Time.	Angle from N point.	Mean Time.	Angle from N. point.	
			p. m.		p. m.		d. h.
April 19	56 Geminorum ..	5·0	9.24	150°	10.6	2 28	7 2
,, 19	61 Geminorum ..	5·7	11.53	1 47	12.30	230	7 5

THE PLANETS.—Mercury (April 1, R.A. 23h 18m; Dec. S. 4° 17′; April 30, R.A. 1h 4m; Dec. N. 3° 56′) is a morning star in Pisces rising at 4.35 a.m. on the 15th, on which date the planet is at greatest elongation of 27° 36′ west, and favourably placed for observation.

Venus (April 1, R.A. 22h 6m; Dec. S. 12° 2′; April 30, R.A. 0h 16m; Dec. S. 0° 4′) is a morning star rising about 4.10 a.m. on the 15th. The planet is diminishing in brightness and presents a gibbous appearance in the telescope, 0·76 of the disc being illuminated.

Mars (April 1, R.A. 17h 59m; Dec. S. 23° 32′; April 30, R.A. 18h 53m; Dec. S. 23° 57′) is a morning star in Sagittarius, rising at 1.8 a.m. on the 15th.

The apparent diameter of the planet is increasing as the opposition, which takes place in July, approaches, being now about 11″. The low altitude of the planet is detrimental to easy observation in small telescopes.

Jupiter (April 1, R.A. 6h 13m; Dec. N. 23° 31′; April 30, R.A. 6h 30m; Dec. N. 23° 25′) is getting more to the west, but is still the most conspicuous object in the evening sky; near the end of the month the planet sets shortly after midnight.

The moon will appear the planet on the evening of the 18th at 7 p.m. The apparent diameter of the planet is diminishing as the distance from the earth increases; the equatorial diameter on the 16th is 35″·6, whilst the polar diameter is 2″·3 smaller.

The following table gives the satellite phenomena observable in this country:—

	Satellite.	Phenomenon.	P.M.'s H. M.	Date.	Satellite.	Phenomenon.	P.M.'s H. M.	Date.	Satellite.	Phenomenon.	P.M.'s. H. M.
Apr				Apr				Apr			
1	I.	Sh. I.	7 20	8	I.	Sh. I.	9 15	15	I.	Tr. I.	9 56
	II.	Tr. I.	7 29		II.	Tr. I.	10 12		I.	Sh. I.	11 10
	I.	Tr. E.	8 20		I.	Tr. E.	10 16	16	I.	Ec. R.	10 44
	I.	Sh. E.	9 38		I.	Sh. E.	11 33	17	I.	Sh. E.	7 57
	II.	Sh. I.	10 7	9	I.	Ec. R.	8 49	18	IV.	Ec. R.	8 24
	II.	Tr. E.	10 20		IV.	Tr. I.	8 58	21	III.	Ec. D.	8 54
	IV.	Ec. D.	11 19		IV.	Tr. E.	11 27	23	I.	Oc. D.	9 14
3	III.	Tr. E.	8 55	10	III.	Tr. I.	9 55	24	I.	Tr. E.	8 42
	III.	Sh. I.	10 56		II.	Ec. R.	9 58		I.	Sh. E.	9 52
7	I.	Oc. D.	10 48	14	III.	Ec. R.	8 2	18	II.	Oc. D.	10 1
8	I.	Tr. I.	7 59					26	II.	Sh. E.	10 14
								28	III.	Oc. D.	8 20

"Oc. D." denotes the disappearance of the Satellite behind the disc, and "Oc. R." its reappearance; "Tr. I." the ingress of a transit across the disc, and "Tr. E." its egress; "Sh. I." the ingress of a transit of the shadow across the disc, and "Sh. E." its egress; "Ec. D." denotes disappearance of Satellite by Eclipse, and "Ec. R." its reappearance.

Saturn (April 15, R.A. 23h 34m; Dec. S. 4° 53′) is a morning star in Pisces rising at 4.26 a.m. on the 15th.

Uranus (April 15, R.A. 18h 55m; Dec. S. 23° 11′) does not rise until about 1.30 a.m. near the middle of the month; he is situated low down in Sagittarius.

Neptune (April 15, R.A. 6h 43m; Dec. N. 22° 13′) is on the meridian about 7 p.m. and sets at 1 a.m. near the middle of the month.

METEOR SHOWERS:—

Date.	Radiant. R.A.	Radiant. Dec.	Name.	Characteristics.
	h. m.			
Apr. 17-May 1	16 0	+47°	τ Herculids	Small; short.
,, 20-21 ..	17 24	+36°	π Herculids	Swift; bl. white.
,, 20-22 ..	18 4	+33°	**Lyrid Shower**	Swift.
,, 30 ..	19 24	+59°	o Draconids	Rather slow.

Minima of Algol may be observed on the 13th at midnight, and on the 16th at 8.49 p.m.

TELESCOPIC OBJECTS:—

DOUBLE STARS.—γ Virginis, XII.h 37m, S. 0° 54′, mags. 3, 3; separation 5″·9. Binary system; both components are yellow, though one is of a deeper hue than the other. An eyepiece of a power of 30 or 40 is required on a 3-in. to effect separation.

Boötis, XIV.h 36m, N. 16° 53′, mags. 4, 6; separation 6″. Requires a power of about 40.

ε Boötis, XIV.h 41m, N. 27° 30′, mags. 3, 6½; separation 2″ 6. Very pretty double, with good colour contrast, the brigher component being yellow, the other blue green.

ξ Boötis XIV.h 47m, N. 19° 31′, mags. 5, 7; separation, 2″·4. Binary; one component being orange, the other purple.

CLUSTERS.—M 3 (*Canes Venatici*) XIII.h 38m, N. 28° 48′. This object, though really a globular cluster of myriads of small stars, appears more like a nebula in small telescopes. It is situated between Cor Caroli and Arcturus, but rather nearer the latter.

XUM